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NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

ROYAL INSTITUTION OF GREAT BRITAIN

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS

VOLUME XXV

1926—1928

LONDON

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1928

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OF THE
Royal Institution of Great Britain

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ALBEMARLE STREET, LONDON, W.1

December 1926

ROYAL INSTITUTION OF GREAT BRITAIN

GENERAL MONTHLY MEETING,

Monday, November 2, 1925.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

John Freeman, M.D.
William Smith Jarratt
Viscount Leverhulme

were elected Members.

The following Letter from Professor Sakurai, elected an Honorary Member at the General Meeting on June 8, in connection with Faraday Benzene Centenary Celebration, was read :—

[Copy]

NATIONAL RESEARCH COUNCIL,
DEPARTMENT OF EDUCATION, JAPAN,
TOKYO,
17*th June*, 1925.

His Grace the President,
The Royal Institution of Great Britain.

YOUR GRACE,

I have had a telegraphic message from Professor M. Katayama, delegate of the National Research Council of Japan to the Centenary of Faraday's discovery of benzene, informing me that the Royal Institution had done me the honour of electing me to be an Honorary Member. I need hardly assure your Grace how very highly I appreciate the honour—an honour bestowed by such a distinguished body as the Royal Institution of Great Britain, and upon such a memorable occasion as the celebration of the Centenary of Faraday's discovery of benzene.

Having had a part of my education in England, and having frequently attended the lectures given by great masters of Science in the hall of the Royal Institution—including Faraday—lectures given by Wurtz and by Helmholtz under the auspices of the Chemical Society of London, it is with particular pleasure that I accept the honour.

The National Research Council of Japan is an organisation representing the whole field of scientific activity in this country, and, as President of that organisation, I feel that the honour done to me is not a mere personal honour, but one bestowed upon Japanese scientists in general.

With feelings of deepest gratitude, I am,
Your Grace's most obedient servant,
(Signed) JOJI SAKURAI,
President.

The Chairman announced the decease of Professor Hugo Hildebrand Hildebrandsson on July 29, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss the Institution and the Science of Meteorology have sustained by the death of Hugo Hildebrand Hildebrandsson, first Professor of Meteorology in the University, and Director of the Meteorological Institute, Upsala, Secretary of the International Meteorological Committee, 1900–1907, Honorary Member of the Royal Institution.

Professor Hildebrandsson was well-known for his meteorological studies on Cloud Formation, in connection with which the International Cloud Year and the International Cloud Atlas, 1896–1897, were published. His extensive researches on centres of action of the atmosphere and problems of the Upper Air are recorded in numerous contributions to the Transactions of the Royal Swedish Academy.

The Managers desire to express on behalf of the Members, their sincere sympathy with the family in their bereavement.

The Special Thanks of the Members were returned to Sir Alfred Yarrow, Bart., F.R.S M.R.I., for his generous Donation of £248 14s. towards the cost of advertising the Lectures for 1925, in "The Times," and to Professor P. Faraday Frankland, C.B.E. LL.D. F.R.S. M.R.I., for his gift of a small Magnet given to Faraday by Logemann, with an autograph letter; and other relics of Faraday and Frankland.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same :—

FROM

Bragg, Sir William, K.B.E. F.R.S. M.R.I.—Modern Views of Physical Science, being a record of the Centenary Meeting of the Franklin Institute, 1924. 8vo. 1924.
Some Causes of Volcanic Activity. By A. L. Day. 8vo. 1925.
Modern Military Engineering. By E. Jadwin. 8vo. 1925.
Field of Research In Industrial Institutions. By E. W. Rice. 8vo. 1925.
Progress and Promise of Engineering. By D. S. Kimball. 8vo. 1925.
A Theory of Colour Production (Franklin Institute). By J. Steiglitz. 8vo. 1925.

British Cotton Industry Research Association—Shirley Institute Memoirs, Vols. I.–III. 8vo. 1922–25.

Burns, G. J., Esq. (the Author)—Some Fingerprints of Light. 4to. 1925. (Typescript.)

Dutt, R. N., Esq., M.A. B.L.—Ruins of Muhammadpur. By L. P. Dutt. 8vo. 1925.
Life of Thakur Phakti-Vinod. By S. C. S. Bhushan. 8vo. 1916.
Revival of Bhagabat Learning in India. 8vo. 1925.

Fleming, Professor J. A., M.A. D.Sc. F.R.S, M.R.I.—Mercury-arc Rectifiers and Mercury Vapour Lamps. 8vo. 1925.

Fondation Edmond de Rothschild, Paris—Fours Electriques et Chimie. Par P. Lebeau. 8vo. 1924.

Hadfield, Sir R. S., Bart., F.R.S. M.R.I. (the Author)—Souvenirs sur mes relations avec la Métallurgie Française. 4to. 1925.
Quelques faits curieux et non encore expliqués, observés dans l'Acier au Manganèse. 4to. 1925.

Lovett, Major-General Beresford, C.B.—A Thermometer with a reformed Fahrenheit Scale.

Melbourne University—The Modern Psychology. By Professor R. J. A. Berry. 8vo. 1921.

National Research Council, Washington—Report of Committee on X-Rays and Radioactivity (Bulletin Vol. X. No. 1). 8vo. 1925.

Norman Lockyer Observatory, The Director—
Spectrum of φ Persei. By W. S. J. Lockyer. 8vo. 1925.
Annual Report. 8vo. 1925.

Parker, W. Rushton, Esq., M.D. M.R.I.—Tombs of the Courtiers and Oxyrhynkhos. By W. M. Flinders Petrie [British School of Archæology in Egypt]. 4to. 1925.

Royal Society, British Empire Exhibition Committee—Phases of Modern Science. 8vo. 1925.

Tokyo, Imperial University—Anniversary Volume dedicated to Professor Hantaro Nagaoka. 8vo. 1925.

UNVEILING OF THE MEMORIAL
of the late
SIR JAMES DEWAR, LL.D. &c.
on Nov. 12, 1925.

MR. H. M. ROSS, Chairman of the Dewar Memorial Committee, in formally handing over the Memorial, said that while Sir James Dewar's memory would always be cherished in that Institution, it had been felt that there ought to be some visible memorial of him in the place in which he worked for nearly half-a-century. It was his desire that there should be no public memorial, but it was thought that to erect a private and domestic memorial in the Institution he served so wholeheartedly would not be contrary to the spirit of his wishes. The memorial it was decided should consist of a plaque placed on the wall of the staircase. He asked the President to unveil the plaque, which had been executed by Sir Bertram Mackennal, and to accept possession of what the Committee hoped and believed would be generally regarded as a work of art, forming a worthy memorial of a man whose love for the arts was second only to his devotion to science.

The DUKE OF NORTHUMBERLAND, the President, in accepting the Memorial on behalf of the Members of the Royal Institution, said the Memorial had been erected to show their grateful recognition of the services rendered to the Institution, to the country, and to the world, by one of the greatest men of science of their epoch. To those who had been privileged to sit at his feet or to work with him the Memorial would speak of his qualities as a man during the forty-six years he worked for the Royal Institution. His life and his example were a priceless legacy to the Institution and would be a lasting source of inspiration to those who came within its walls.

Sir J. J. Thomson, Master of Trinity College, Cambridge, said no one connected with the Institution had ever delighted its members with experiments of such amazing beauty and extraordinary difficulty as Sir James Dewar. He was an artist to his finger-tips, and introduced a dramatic element into practically every experiment. He had found his true home there, for he was essentially an investigator and a pioneer. Three of his discoveries could not be passed over in any tribute, however brief. It was undoubtedly to Dewar that they owed the use of liquefied gases as a physical agent. The vacuum flask was not only an important scientific instrument, but had added largely to the amenities of life to many who were unaware to whom they owed it. Of special importance was his discovery of the method of producing high vacua by means of charcoal cooled by liquid gas, to which the advance of modern physics was to no small degree due. The rapid progress of modern science owed more perhaps to that discovery than to any other. They were all proud that Dewar's great discoveries were made within the walls of the Royal Institution.

GENERAL MONTHLY MEETING,

Monday, December 7, 1925.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Chairman moved the following Resolution of Condolence with His Majesty the King and His Royal Highness the Prince of Wales on the decease of Her Majesty Queen Alexandra, which was carried unanimously, all the Members present standing :—

To THE KING'S MOST EXCELLENT MAJESTY.

The Humble Address of the
Members of the Royal Institution of Great Britain.

The Members of the Royal Institution of Great Britain, in General Meeting here assembled, desire most respectfully to offer to His Majesty the King (Patron of the Royal Institution) and His Royal Highness the Prince of Wales (Vice-Patron of the Royal Institution) the expression of their unfeigned sorrow at the loss which His Majesty, His Royal Highness, and the Nation have sustained by the death of Her Majesty Queen Alexandra, and they ask to be allowed to tender their heartfelt sympathy and condolence with His Majesty and His Royal Highness the Prince of Wales in their bereavement.

William Glynes Bruty,
Percy Carter Buck, Mus.Doc.
John Ratcliffe Cousins, LL.B.
James Scott Duckers,
Arthur Edmunds, C.B. M.S. F.R.C.S.
Michael Alexander Greig,
Edmund Marsden,
Mrs. Murray,
Lieut.-Col. Berkeley Crosbie Quill, C.B.
Albert L. Reckitt,
Miss Elizabeth M. Stear,
James Alexander Stevenson,
Henry B. Gordon Warren,

were elected Members.

The Special Thanks of the Members were returned to Dr. W. Rushton Parker for his generous Donation of £100 towards the improvement of the Library.

The following Lecture Arrangements Before Easter, 1926, were announced :—

PROFESSOR SIR WILLIAM BRAGG, K.B.E. LL.D. D.Sc. F.R.S. M.R.I., Fullerian Professor of Chemistry. Six Lectures (Illustrated), adapted to a Juvenile Auditory, on OLD TRADES AND NEW KNOWLEDGE: 1. THE TRADE OF THE SAILOR; 2. OF THE SMITH; 3. OF THE WEAVER; 4. OF THE DYER; 5. OF THE POTTER; 6. OF THE MINER. On Dec. 29 (*Tuesday*), Dec. 31, 1925; Jan. 2, 5, 7, 9, 1926. (100th Course.)

JAMES A. CROWTHER, Esq., M.A. Sc.D. F.Inst.P., Professor of Physics, University College, Reading. Two Lectures on X-RAYS AND LIVING MATTER. On *Tuesdays*, Jan. 19, 26.

ERIC K. RIDEAL, Esq., M.B.E. D.Sc. F.C.S. M.R.I., Owen Jones Lecturer on Physical Chemistry, University of Cambridge. Two Lectures on SURFACE ACTION. On *Tuesdays*, Feb. 2, 9.

JOSEPH BARCROFT, Esq., C.B.E., F.R.S., Fellow of King's College, Cambridge; Fullerian Professor of Physiology, R.I. Four Lectures on THE EGG: 1. INTRODUCTORY; 2. THE SHELL; 3. THE YOLK; 4. THE WHITE. On *Tuesdays*, Feb. 16, 23, March 2, and *Monday*, March 8.

CECIL H. DESCH, Esq., D.Sc. F.R.S., Professor of Metallurgy, University of Sheffield. Two Lectures on THE GROWTH OF CRYSTALS. On *Tuesdays*, March 16, 23.

CHARLES J. PATTEN, Esq., M.A. M.D. Sc.D. F.R.A.I., Professor of Anatomy, University of Sheffield. Two Lectures on 1. THE LANGUAGE OF BIRDS; 2. THE BREEDING FACTOR IN BIRDS. On *Thursdays*, Jan. 21, 28.

John L. Myres, Esq., O.B.E. D.Sc. F.S.A., Fellow of the British Academy. Three Lectures on Who were the Greeks? On *Thursdays*, Feb. 4, 11, 18.

C. D. Ellis, Esq., Ph.D., Fellow of Trinity College, Cambridge. Three Lectures on The Atom of Light and the Atom of Electricity. On *Thursdays*, Feb. 25, March 4, 11.

John Holland Rose, Esq., Litt.D., Vere-Harmsworth Professor of Naval History, University of Cambridge. Two Lectures on The Indecisiveness of Modern Warfare. On *Thursdays*, March 18, 25.

Henry Balfour, Esq., M.A. F.R.S., Curator of the Pitt Rivers Museum, Oxford. Two Lectures on 1. The Evolution of Currency and Coinage; 2. The British Coracle, or Skin-Covered Boat, and its Affinities. On *Saturdays*, Jan. 23, 30.

Sir Walford Davies, Mus.Doc. LL.D., Professor of Music, University College of Wales, Aberystwith. Two Lectures (with Musical Illustrations) on The Triad and the Perfect Fourth: 1. Their Nature; 2. Their Uses from Hucbald to the Present Day. On *Saturdays*, Feb. 6, 13.

George Macdonald, Esq., C.B. LL.D. D.Litt. F.B.A., Permanent Secretary, Scottish Education Department. Two Lectures on Roman Britain. On *Saturdays*, Feb. 20, 27.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. F.R.S. M.R.I., Professor of Natural Philosophy, R.I., and Cavendish Professor of Experimental Physics, University of Cambridge. Four Lectures on The Rare Gases of the Atmosphere and their Importance in Atomic Theory. On *Saturdays*, March 6, 13, 20, 27.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Ascroft, R. W. (*the Author*)—The Conservation of the Nation's Vegetation. 8vo. 1925.

Lister Institute of Preventive Medicine—Catalogue of the National Collection of Type Cultures. Second Edition. 8vo. 1925.

Manchester Steam Users' Association—Boiler Explosion Acts, 1882 and 1890. Board of Trade Reports, No. 2655 to No. 2712A. 4to. 1925.

WEEKLY EVENING MEETING.

Friday, January 29, 1926.

SIDNEY G. BROWN, F.R.S. M.I.E.E., Manager, in the Chair.

SIR WILLIAM B. HARDY, F.R.S., Fellow and Tutor of Gonville and Caius College, Cambridge.

Films.

THE title of this discourse is perhaps misleading. It has, I find, suggested to my friends a popular form of entertainment. Let me hasten to set this right. I shall have nothing to say about the cinema! My subject is something quite different—namely, those thin films of matter, familiar to all in the form of soap bubbles or lubricating films of oil.

The subject is of great scientific interest. You all of you know of the three states in which matter can exist—solid, fluid and gas or vapour. These films are no less than a fourth state, because, choose what physical constant we may, it will have a different value for any particular kind of matter in this state to what it has in any of those other states which are more easily apprehended by the senses.

Life itself depends upon this fourth state of matter. There is a film spread over the surface of each living cell which seems to control the passage of substances into or out of the cell. This film is actively maintained by the expenditure of energy on the part of the cell. The new technique of microdissection, by which living cells so small as to be almost or quite invisible can be dissected, has increased our knowledge of this surface film. If it be punctured at one place, the living matter in the neighbourhood of the puncture becomes curdled in appearance and dies, but the membrane grows in at the back of this dead substance cutting it off from the rest of the cell. This is the fundamental surgery of life.

I cannot hope to do more in one hour than deal in haphazard fashion with this vast subject. I propose to begin with an experiment which, in spite of its simplicity, shows how ubiquitous films are, and how our most elementary impressions of the external world depend upon them.

Take, for example, smoothness. It is not a property of solid matter in mass, but of this fourth state of matter. A tea-cup has the delicate velvety feel of a polished surface; but neither porcelain nor ware is really smooth in that sense. Their surface, like that of all other naturally occurring surfaces, is covered by a film of greasy matter, which can come from the atmosphere or from the "clean" cloth with which the object has been dried. If that film be removed the surface feels harsh and rough because, to use the engineer's phrase, one's finger-tips, if they are freshly washed, seize to it.

It is not possible here and now to remove the film. The necessary procedure would take too long, and in any case the film would quickly re-form in this atmosphere. I can, however, destroy its effectiveness by taking advantage of a curious property of water. That substance is not only not a lubricant for vitreous surfaces, but it is also an anti-lubricant in that it destroys the effect of the natural lubricating film. All I have to do, therefore, is thoroughly to wet the surfaces of the tea-cup and saucer, and the tea-cup ceases to slide in the saucer.

A tea-cup suggests a storm, and that suggests the curious power which oil has of smoothing the sea. The oil spreads over the surface of the water until the layer is only about the five-millionth of a millimetre in thickness. A figure of that kind is apt to mean little; I will therefore try to give you an impression of the minute quantity of oil needed in another way. In 1919 an oil ship was wrecked inside the Lizard. The oil-tanks were burst open and the oil rapidly escaped. There has been no sensible quantity of oil in the wreck for the last six years, yet sufficient still escapes to the surface of the sea to produce an obvious "smooth" for a mile or more to leeward. The effect of a film of oil of quite invisible thickness upon the sea is very real. A vessel labouring in a sea-way or running before a gale can, and does, find some measure of safety by streaming bags filled with oil to windward, and Pliny records how the oyster-fishers used oil to calm the surface of the sea so that they were more easily able to work.

It is obvious that the presence of this oil film cannot seriously modify the energy of great seas, say, a quarter of a mile from crest to crest; but when seas enter a "smooth" they change their character with dramatic suddenness. They lose their viciousness, and the moment they are in the "smooth" take the character of those relatively harmless undulations which do not break on to a vessel, but merely make her roll and pitch. The question how the oil film, so tenuous as to be of invisible thickness, curbs the seas is an interesting one, and the attempt to answer it will inevitably introduce us to the chief properties of films on water.

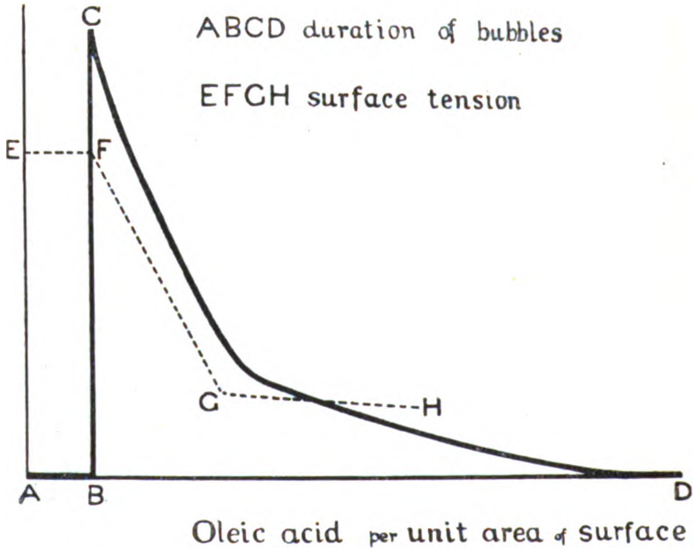
In the late nineties a most ingenious method of demonstrating the existence of films on water, and of controlling them for experimental purposes, was devised by a German lady, Fräulein Pockels. I think I may say without exaggeration that the immense advances in the knowledge of the structure and properties of this fourth state of matter which have been made during this century are based upon the simple experimental principles introduced by Miss Pockels. I propose now to make use of her method. Here is an oblong trough of metal filled with water. On the surface of the water, quite invisible to you because it is even thinner than the invisible dead black portion of a soap film, there is a layer of greasy contamination. If I lay upon the trough a strip of glass or metal so that it touches and is wetted by the water, and move it along, I can compress the superficial film in front and expand it behind. Both processes are easily rendered visible by scattering lycopodium dust on the surface.

The capacity which these films have of expansion is easily shown by sweeping the natural film to one end, thus leaving a tolerably clean surface of water behind. Some lycopodium dust is now placed at one end and the surface touched with a platinum wire, the extreme tip of which has just been dipped into an oil. The dust particles are swept swiftly away in front of the advancing film of oil, although the film itself is absolutely invisible.

The film tends to spread, but the surface of the water in virtue of its surface tension tends to contract. It is this same surface tension which rounds up drops of fluid to spheres, or as near an approach to the spherical shape as other forces which may be operating, such as gravity, permit. There are therefore opposing influences: the tendency of the water to contract, opposed by the tendency of the film to expand, with the result that composite surfaces of oil and water have a surface tension less than that of pure water. Composite fluid surfaces have also an enhanced mechanical stability. When a ring of wire a few centimetres in diameter is withdrawn from clean water no film is formed across it, but when the surface of the water is coated with oil it acquires the property of forming free films, which may have an endurance comparable even with that of a soap bubble. In the figure the curve E F G H gives the surface tension plotted against the quantity of oleic acid per unit area of surface, and the curve A B C D gives the duration of bubbles which have been formed on the surface by allowing air to escape slowly and regularly from an orifice within the trough. It will be noticed that the bubbles are most stable when the film of contamination is just dense enough to begin to alter the surface tension, and that the bubbles have no stability when contamination is either vanishingly small or very great.

Do these properties of composite surfaces, namely, the lowered surface tension and the increased mechanical stability, explain the calming of the sea? In my opinion the answer is "No," but current doctrine would perhaps say "Yes." It has been pointed out that the special capacity of composite surfaces to resist extension and their mechanical stability, which is only another special aspect of the same thing, tends to prevent the inevitable expansion of the surface which occurs when a wave is formed. This has been held to be a sufficient explanation. I do not think it is, and for two reasons. The first is that the surface of the sea always is contaminated by something which lowers its surface tension and gives to it a remarkable measure of mechanical stability. One of the most striking aspects of a heavy gale is the "windrows," which are due to the foam formed when a sea breaks being blown by the wind in long lines over the surface. Foam after all is no more than a collection of bubbles; obviously therefore these naturally formed bubbles have great stability.

The surface of the sea is already covered by a film of matter the nature of which must remain uncertain. Sometimes it is composed of substances, like saponin of vegetable origin, derived from the masses of seaweed flung upon the coast. Foam of this kind is remarkably stable. I have seen it on the day following an on-shore gale knee-deep in the hollows above Flamborough Head. The bruising and shattering of seaweed is however a coastal happening and "windrows" are deep-sea phenomena.



The true explanation was, I think, furnished by Benjamin Franklin in 1773. His discussion is worth reading. It has the spacious dignity and charm which the hurry and specialisation of to-day have of necessity banished from scientific papers. He tells how he was at sea in 1757 with a convoy of 96 sail, the wind being very fresh, and how he noticed a "smooth" in the wake of two of the vessels. He enquired the cause of one of the officers and was told with some degree of contempt, it being a thing which every fool should know, that the "smooth" was due to the fact that the cook had just thrown greasy water over the side. In those days tallow was used to coat the bottoms of vessels to keep them clear of growth, and Franklin also notes the "smoothness" in the wake of vessels which had been freshly tallowed.

Franklin's explanation is based entirely upon friction. The oil makes the sea so very smooth that the wind cannot "catch upon it." I confess Franklin's explanation did not appeal to me at first, but I believe he is right. The comparative safety of a "smooth" is due not to the fact that the seas in it are sensibly smaller than those outside of it, but to the fact that they have been deprived of their viciousness. Now the viciousness of a sea, the degree of danger it carries to the mariner, is measured by its instability. It is when the head of the sea topples over and becomes a mass of water moving with a high velocity that it is dangerous. Within the limits of a "smooth" produced by oil the seas cease to break, or to "crack" as Cornish fishermen say. The wind not only drives a sea forward by its horizontal pressure, but also draws the crest upwards by friction against the surface of the wave.

If the friction between the air and the water be greatly reduced, the wind fails to lift the crest of a wave to the point at which it is blown bodily over by the horizontal pressure. The wave then sinks down to a relatively harmless "swell."

The "catch" of the wind upon the waves is not however confined simply to a direct frictional pull, and here it is that the surface tension phenomena perhaps come in. It is easy to convince oneself that an oil film prevents the formation of ripples—that is, of the very smallest kind of wave. When there is no oil film a great wave carries countless ripples and wavelets each of which gives the wind a direct thrust on the surface. It is to the suppression of ripples and wavelets that the characteristic smooth appearance is due, and when they cease to be formed the chief "catch" of the wind upon the sea is lost.

[W. B. H.]

GENERAL MONTHLY MEETING,

Monday, February 1, 1926.

Ernest Clarke, M.D. F.R.C.S., Manager, in the Chair.

Henry John Heckle,
George William Clarkson Kaye, O.B.E. D.Sc.
Mrs. Shaw-Stewart,
Robert M. Weston,
Harry James Yates,

were elected Members.

The Special Thanks of the Members were returned for an Anonymous Donation of £500 to the Fund for the Promotion of Experimental Research.

It was unanimously

Resolved, That the Members of the Royal Institution assembled in General Meeting this day congratulate Sir James and Lady Crichton-Browne on his progress, and express the hope that his convalescence will soon be well established and his recovery complete.

The following Presents received since the last meeting were laid on the table, and the Thanks of the Members were returned for the same :—

FROM

Lorentz, Dr. H. H., Secretary, Société Hollandaise des Sciences—Œuvres Complètes de Christiaan Huygens. Tome XV. 4to. 1925.
Parker, Dr. W. Rushton, M.D. M.R.I.—The Book of Birds. 8vo. 1925.
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Index, Vols. XXXVI.-L. 8vo. 1925.
Willmott, Miss E., M.R.I.—Under the Searchlight. By V. Douglas Pennant. 8vo. 1922.

The following Publications of Scientific Societies and Periodicals have been presented or received in exchange during 1925 :—

British Museum Trustees—Catalogue of Sea Urchins. 8vo. 1925.
Study of Minerals, 16th Edition. 8vo. 1925.
Guide to Fossil Plants. 8vo. 1925.
Index Animalium—Sectio Secunda, Part 6. 8vo. 1925.
Economic Series: II. Furniture Beetles, 2nd Edition. 8vo. 1925.
Catalogue of Engraved British Portraits, Vol. VI. 8vo. 1925.
Catalogue of Vases, Vol. I. Part 1. 8vo. 1925.

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  - Report on Cape Observatory, 1924. 4to.
  - Annals of the Cape Observatory, Vol. VIII. Parts 7-8; Vol. XIII. Part 1. 4to. 1925.
  - Cape Astrographic Zones, Vol. VIII. 4to. 1925.
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WEEKLY EVENING MEETING,

Friday, February 5, 1926.

Sir Edward Pollock, F.R.C.S., Manager and Vice-President, in the Chair.

George Gordon, Merton Professor of English Literature, University of Oxford.

Shakespeare's English.

[No Abstract.]

WEEKLY EVENING MEETING,

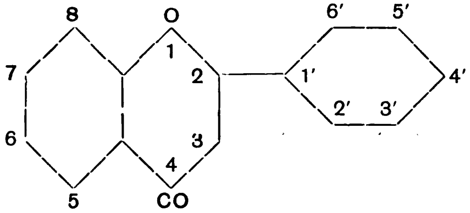
Friday, February 12, 1926.

Sir Ernest Moon, K.C.B. K.C. LL.B., Manager and Vice-President, in the Chair.

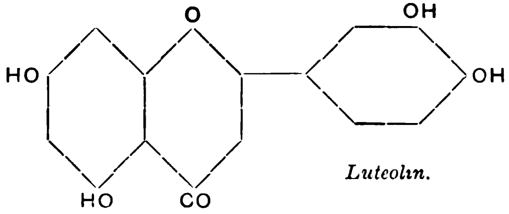
Robert Robinson, D.Sc. F.R.S., Professor of Organic Chemistry, University of Manchester.

Chemistry of Red and Blue Colouring Matters of Flowers.

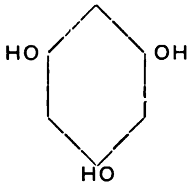
Prior to the work of Richard Willstätter, the publication of which began in 1913, there was a striking deficiency in our knowledge of the nature of the bright red and blue, violet, purple, mauve and magenta colours of flowers, fruits and blossoms. These water-soluble anthocyanin pigments occurring in the cell sap were already distinguished from the water-insoluble plastid pigments, such as carotin, xanthophyll and chlorophyll, which occur alongside protoplasmic bodies, and many of their superficial reactions were known. Glenard (1858), Senier (1878), Heise (1889), and Glan (1892) examined various imperfectly purified anthocyanins and found these to contain carbon, hydrogen and oxygen only; whilst Griffiths (1903) obtained the geranium pigment in a crystalline condition, and came to the same conclusion regarding its elementary composition. Moreover, the botanist Molisch showed in 1905 that in many cases the pigments exist in the living plant in a microcrystalline condition, and followed up this observation by demonstrating the crystallisability of several of these colouring matters on a small scale outside the plant. Molisch considered that the crystalline pigments contained sugar in a combined form, but when Grafe succeeded in reproducing Molisch's experiments on a larger scale with pelargonium petals, he failed to recognise that the crystalline colouring matter he had obtained was a glucoside. Willstätter and Everest (1913) examined anthocyanins in general, and the pigment of the cornflower in particular. They found that the anthocyanins are glucosides having both basic and acidic properties. The anthocyanin of the blue cornflower they called "cyanin," and showed that it exists in the plant as its potassium salt. It was, however, isolated by taking advantage of its power of forming compounds with acids rather than with alkalies. These salts—for example, potassium cyanin and cyanin chloride—could be crystallised, the latter with much the greater ease. The action of boiling 20 per cent. hydrochloric acid on cyanin chloride splits off the sugar groups and gives glucose (2 mols.) and cyanidin chloride, which crystallises from the solution. The name "anthocyanidin" is applied to all the sugar-free pigments which can be obtained by detaching sugar from the anthocyanins. Only in the case of the colouring matter of the skins of black grapes is the anthocyanin—namely, oenin—supposed to be accompanied in nature by the corresponding anthocyanidin—namely, oenidin. Anthocyanin chlorides are much more soluble in water, thus partaking of the character of the sugar contained in their molecules, than are the anthocyanidin chlorides. The latter are completely extracted from aqueous solution by amyl alcohol, whilst the anthocyanins are only removed to a certain extent, usually to a very small extent, by this solvent. Subsequent work showed that cyanin is contained in red as well as blue flowers, and the following figures give the percentage of the colouring matter in the dried petals: Blue cornflower, 0·75; deep violet-blue cornflower, 3·6; deep bordeaux cornflower, 13–14; deep red dahlia, 20; *Rosa gallica*, 2. About 50 per cent. of the amount present in deep red dahlia petals can be isolated in the following manner. The fresh flowers are extracted with acetic acid, methyl alcoholic hydrochloric acid, and 1½ volume of ether being then added. This precipitates the chloride, which is insoluble in ether, and the salt may then be separated and crystallised in the ordinary manner from a solution in 7 per cent. hydrochloric acid. It should be added that this process is unusually straightforward owing to the high percentage of colouring matter in the flowers, a tribute to the effective work of the florists. Willstätter and Nolan, who prepared cyanin chloride from petals of *Rosa gallica*, showed that cyanidin chloride had the composition $C_{15}H_{11}O_6Cl$. Willstätter and Bolton isolated the anthocyanin, pelargonin, from the scarlet pelargonium, and found that the sugar-free pigment, pelargonidin chloride, had the composition $C_{15}H_{11}O_5Cl$; whilst Willstätter and Mieg obtained delphinin from the wild purple larkspur (*Delphinium consolida*), and found that it could be degraded to delphinidin chloride, $C_{15}H_{11}O_7Cl$. Every anthocyanin pigment which has been closely examined up to the present time has been found to be derived from one of the three anthocyanidins already mentioned, and the variety of flower colours is due to what may be described as the various exterior fittings of the molecules, as well as to the nature of the sap. If the latter is acid the colour tends to be red, if it is neutral the flower may be violet or purple, and if it is alkalescent the flower will be blue. Anthocyanins derived from pelargonidin occur in the scarlet *Pelargonium zonale*, the purple-red summer aster, the scarlet *salvia*, and in radishes. Cyanidin derivatives are very widespread, and have been detected in the cornflower, rose, dahlia, chrysanthemum, aster, peony, poppy, and in the fruits of the cherry, cranberry and sloe, as well as in many other flowers, fruits and berries. Delphinidin is obtained from the anthocyanins of grapes and bilberries, and of the flowers of delphinium, hollyhock, petunia, wild mallow and viola. It is obvious that the problem of the molecular structure of the anthocyanins must be attacked by way of that of the simpler anthocyanidins, and in this matter conjectures confirmed by crucial experiments played a leading part. In the first place the analogy in composition between the anthocyanidins and the yellow plant colouring matters of the flavone and flavonol series could not be overlooked. Thanks to the labours of A. G. Perkin, von Kostanecki, and other chemists, we already possessed a knowledge not only of the wide distribution of these anthoxanthins in nature, but also of the details of their molecular structure. They are all of them derivatives of flavone, $C_{15}H_{10}O_2$, a compound which was synthesised by Kostanecki, and



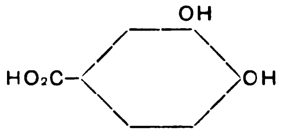
subsequently discovered by Müller to be the chief constituent of the farina of *Primula pulverulenta.* The other naturally occurring flavones have OH instead of H in several of the possible positions, and this substitution confers on them a weakly acidic character. It should be noticed that many of the hydroxylated flavones are weak bases and form readily decomposable salts with strong acids. We pass in review the established constitution of a few of the members of the group. Chrysin, $C_{15}H_{10}O_4$, from poplar buds has OH instead of H in positions 5 and 7 ; apigenin from parsley has the formula $C_{15}H_{10}O_5$, aud the OH groups are in 5, 7 and 4′; luteolin from weld is $C_{15}H_{10}O_6$, and has OH groups in 5, 7, 3′ and 4′. Scutellarein from *Scutellaria altissimma* has its four OH groups in 5, 6, 7 and 4′. The flavones with an OH group in position 3 are called flavonols, and are more widely distributed than the flavones. Kæmpferol, $C_{15}H_{10}O_6$, has OH groups in 3, 5, 7 and 4′ ; it can be obtained from *Delphinium consolida.* Quercetin, $C_{15}H_{10}O_7$, with OH groups in 3, 5, 7, 3′ and 4′, is found widespread in nature. Derivatives of it occur, for example, in the inner bark of *Quercus discolor* Ait., in the leaves of *Eucalyptus macrorhyncha,* in rue, capers, buckwheat, clover flowers, cotton flowers, wallflowers, horse-chestnut, etc Myricetin, $C_{15}H_{10}O_8$, with OH groups in 3, 5, 7, 3′, 4′ and 5′, is obtained from *Myrica nagi,* the ever-green box-myrtle of China, and other plants. All these flavones and flavonols have been artificially prepared in the laboratory, and, so far as is known, they all occur in the plant in combination with sugars. In order to appreciate the analogy in composition between the flavones and anthocyanidins, it is merely necessary to note that luteolin combines with hydrochloric acid to form a hydrochloride which has the same empirical formula, $C_{15}H_{11}O_6Cl$, as cyanidin chloride. Now when luteolin is fused with caustic alkalies its molecule is destroyed, but not so completely that none of the fragments can be recognised. The middle ring of atoms is wholly broken up by this vigorous treatment, but the benzene rings to some extent withstand the strain. The products which can be isolated are called phloroglucinol and protocatechuic acid; they are substances which can be artificially prepared in several different ways, and the position which they occupy in the system of organic chemistry is perfectly well understood. The first indication that the hypothesis of a close relation between the flavones and the anthocyanidins is correct was the fact that when cyanidin chloride was fused with alkali it also yielded phloroglucinol and protocatechuic acid.



Luteolin.



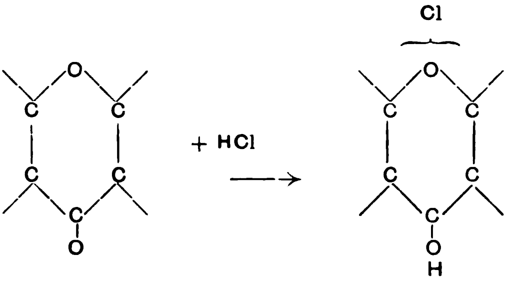
Phloroglucinol.



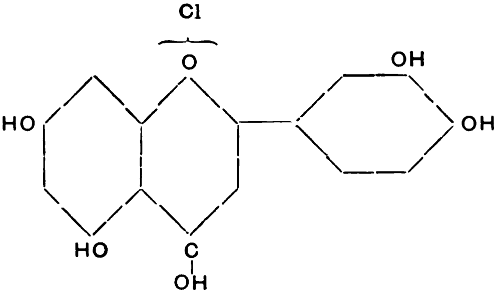
Protocatechuic Acid.

A second group of researches precedent to those of Willstätter and his collaborators was concerned with the basic character of non-nitrogenous carbon compounds containing oxygen. Many substances of this type exhibit a weak affinity for acids, but Collie, Werner, Baeyer, and others showed that quite stable so-called oxonium salts are formed by a group of compounds the molecules of which include a ring of five carbon atoms and an oxygen atom. Such a ring is contained in flavone and its derivatives.

In accordance with modern ideas the salt formation is represented by the above scheme, and the bracket between Cl and O is a symbol expressing the view that the chlorine atom on the one hand, and the rest of the molecule on the other, are electrically charged in opposite

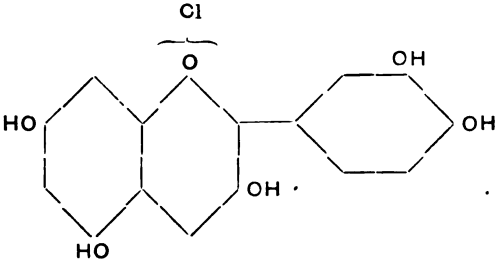


senses. It was then found that even more stable salts can be obtained by adopting such methods of synthesis as will rise to compounds in which the OH of the above formula is replaced by H. Oxonium bases of this type have been recently encountered that are strong enough to form stable acid carbonates. If now we consider what modification of the constitution of luteolin hydrochloride could supply an explanation of the nature of the isomeric cyanidin chloride,

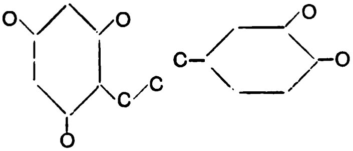


Luteolin hydrochloride.

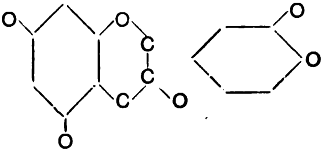
we perceive at once that the expression annexed demands serious consideration. Three independent syntheses of cyanidin chloride



have proved conclusively that this formula does, in fact, represent the constitution of the substance. In Willstätter's syntheses the bricks were



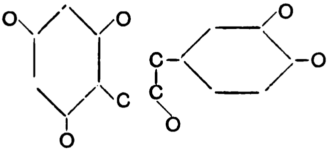
or



(I)

(II)

whilst in those which were performed in St. Andrews and Manchester the bricks were



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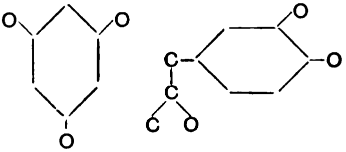

(III)

(IV)

Method III is the only convenient and general process.

Pelargonidin chloride differs from cyanidin chloride in that it lacks the OH group in position 3′, whilst delphinidin chloride has OH groups in 3′, 4′ and 5′, as well as in 3, 5 and 7. The former anthocyanidin has been artificially prepared by methods (II), (III) and (IV), and the latter by two variations of method (III). The nucleus common to the anthocyanidins we call conveniently "flavylium," and denote the position of attached groups by the numbering system already adopted for flavone. A large number of flavylium salts have been prepared in order to obtain information as to the influence of the position of OH groups on the properties of the substances.

(1) As the number of OH groups increases, the colour of acid solutions passes from nearly colourless, through yellow, orange and red, to bluish red.

(2) If there is an OR group in position 3, solutions of the salts are decolorised on dilution, and in most cases the colour is restored on the addition of acid.

(3) If there is an OH group in position 7, leaving positions 5 and 8 free, the salt may exhibit fluorescence in alcoholic solution.

(4) Only certain combinations of OH groups in at least four positions (two of which must be 3 and 4′) confer on the salt the power of dissolving in aqueous sodium carbonate solution to a pure blue solution. Such combinations are: 3, 5, 7 and 4′, and 3, 7, 3′ and 4′.

(5) An intense colour reaction with ferric chloride (blue in alcohol, violet in water) is only exhibited when two OH groups occupy adjacent positions, usually 3′ and 4′, on the nucleus. A typical example of the application of this knowledge to a structural problem is concerned with the anthocyanin of the peony. This substance, called peonin, is readily decomposed by boiling 20 per cent. hydrochloric acid to glucose and peonidin chloride, $C_{16}H_{13}O_6Cl$. This is CH_2 more than cyanidin chloride, and it can easily be shown that peonidin chloride is cyanidin chloride in which OH is replaced by OCH_3. Cyanidin iodide and CH_3I are obtained from peonidin by the action of hydriodic acid. Synthetical compounds of this series containing CH_3 groups are numerous, and we know that, whilst OCH_3 is about as powerful as OH in connection with the effect on the colour of the salt in acid solution, its influence on the ferric chloride reaction and the colour in alkaline solution is negligible.

Now, peonidin salts do not exhibit an intense ferric chloride reaction. Therefore the OCH_3 is at 3′ or 4′. The alkali reaction is however as strong as that of pelargonidin, and therefore OH in 4′ is indicated and the OCH_3 is at 3′. This view of the structure of peonidin chloride has been confirmed by synthesis (Method III). The position occupied by sugar groups in the anthocyanins may be elucidated in a similar manner. There are, for example, two diglucosides of cyanidin—namely, cyanin and mecocyanin (from the poppy). Both give intense ferric chloride reactions, and therefore have OH groups at 3′ and 4′. The sugar groups must be attached to 3, 5 or 7, and if we blocked up any two of these with C_6 sugar groups there would be no explanation of the strong alkali colour reactions of cyanin and mecocyanin. One OH must suffice to hold both molecules of sugar in each case, and we must be dealing with $-OC_{12}$ rather than with two $-OC_6$ groups. Cyanin gives a pure blue with sodium carbonate, but mecocyanin yields in this way a violet which changes to blue on the addition of sodium hydroxide. The synthetical flavylium salt with four OH groups in 3, 7, 3′ and 4′ has the colour reactions of cyanin, whilst that having OH groups in 5, 7, 3′ and 4′ has reactions almost identical with those of mecocyanin. It is, therefore, very probable that the $-OC_{12}$ group in mecocyanin is in position 3 or 7, whilst in cyanin it is in position 5 or 7. For other reasons there is little doubt that the $-OC_{12}$ group in mecocyanin is in position 3. Artificial anthocyanins—that is, flavylium salts with attached glucose molecules similar, but not yet identical, with the natural anthocyanins—have now been prepared in the Manchester laboratories (Method III). The restrictions which nature has laid on herself in the matter of anthocyanidins seem curious in comparison with the lavish production of variations in the structure of the closely related flavones and flavonols. The flavylium salts corresponding to all the naturally occurring flavones and flavonols must be regarded as potential natural anthocyanidins, and they have almost all been synthesised. An indication that a derivative of one of these exists in nature has been obtained from a most unexpected quarter—namely, the rare red pigments prepared by Indians of Central America. Apparently, in preparing the colour, the leaves of *Bignonia chica*, a bushrope, are boiled with water, and particles of the bark known as "aryane" are added to the decanted liquid, causing the precipitation of the colouring matter. In 1914 Professor A. G. Perkin examined a specimen of "carajura" obtained from Manaos on the Orinoco, and isolated a crystalline red compound which he called carajurin. This has basic properties, and is decomposed by hydriodic acid, with formation of CH_3I (2 molecules) and carajuretin iodide. Subsequent investigations have shown that this salt has the same composition as pelargonidin iodide, and that it differs from the latter only in having an OH group in position 6 instead of in position 3. Carajuretin is, in fact, a fourth anthocyanidin, and is related in constitution to the flavone scutellarein. Naturally we are in the dark as to its mode of occurrence in the plant, but the method of preparation, which has been reported, is strongly suggestive of the hydrolysis of a glucoside by means of an enzyme. Few anthocyanins more urgently call for further investigation than that of the funnel-shaped violet flowers of *Bignonia chica.*

[R. R.]

WEEKLY EVENING MEETING,

Friday February 19, 1926.

Sir Edward Pollock, F.R.C.S., Manager and Vice-President, in the Chair.

The Hon. John W. Fortescue, C.V.O. LL.D.,
Librarian at Windsor Castle.

George III. in his Papers.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 26, 1926.

Sir Edward Pollock, F.R.C.S., Manager and Vice-President, in the Chair.

C. Hagberg Wright, LL.D., Secretary and Librarian, London Library.

Nicolas de Peiresc and his Circle: The Story of a 17th Century Patron of Learning.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, March 1, 1926.

Sir Edward Pollock, F.R.C.S., Manager and Vice-President, in the Chair.

Miss May Smithells Barlow, B.A.
William Augustus Caspari, D.Sc.
Sir James George Frazer, O.M. D.C.L. F.R.S. F.B.A.
Lady Frazer,
William Hawthorne,
Reginald Hewlett Hayes, M.R.C.S. L.R.C.P.
Charles Judson,
Miss Isabel Ellie Knaggs, Ph.D.
Lady Jane Lindsay,
Alexander Herbert Macdonald,
Robert Nelson,
John Henry Reynolds, F.R.A.S.
Reginald Soames,
Rev. Charles Robert Shaw Stewart, M.A.
Major David Thompson, R.A.
Miss Mary Walters,

were elected Members.

The Chairman announced the decease on February 21 of Dr. Heike Kamerlingh Onnes, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Institution has sustained by the death of Professor H. K. Onnes, Ph.D., Nobel Prize 1913, Rumford Medal 1916, Foreign Member of the Royal Society, Honorary Member of the Royal Institution, Director of the Physical Laboratory of the University of Leiden.

Dr. Onnes carried out a long series of important investigations on Low Temperature Problems in the Cryogenic Laboratory at Leiden, culminating in the liquefaction of Helium. In the course of his researches he was enabled to confirm the theory that at extreme low temperatures the electrical resistance of certain metals disappears.

The Managers desire, on behalf of the Members, to express their sincere sympathy with Madame Onnes and the family in their bereavement.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Clarke J. W. (the Publisher)—Hampton's Scholastic Directory. 33rd Edition. 1925–26. 8vo. 1926.

National Research Council, Washington.—Marine Structures : Their Deterioration and Preservation. 8vo. 1924.

Parker, Dr. W. Rushton, M.D. M.R.I.—Buttons and Design Scarabs. By Sir W. M. Flinders Petrie (British School of Archæology in Egypt). 4to. 1925.

Toronto University.—A New View of Surface Forces. By W. Taylor. 8vo. 1925.

WEEKLY EVENING MEETING,

Friday, March 5, 1926.

SIR EDWARD POLLOCK, F.R.C.S., Manager and Vice-President, in the Chair.

SIR HENRY P. MAYBURY, K.C.M.G. C.B. M.Inst.C.E.,
Director-General of Roads Department, Ministry of Transport.

London Traffic.

THE writer is deeply appreciative of the privilege the authorities of the Royal Institution have extended to him by permitting him to submit to this distinguished audience some of the problems appertaining to the Transportation Services in Greater London.

In this great home of scientific research, famous for its discoveries and inventions, and having on its membership roll the names of men recognised throughout the world as being pre-eminent in their respective professions, I do indeed esteem it an honour to be permitted to give a lecture upon a subject which is rarely considered from a scientific point of view. I shall hope to show you that the satisfactory handling of so vast and costly a problem as that connected with the transport facilities of this great Metropolis cannot possibly be solved unless it be approached upon scientific lines.

Before proceeding to apply remedies, it is essential to have a full appreciation of the problem and to know something of its history.

The history of the City of London and the growth of the outer ring, now represented by an area within a circle extending to 25 miles from Charing Cross, may be assumed to be the history of London traffic. Records of the Livery Companies and Guilds of the City, and minute books of the Vestries and Governing Authorities of the past, go to show that there has ever been a street and highway problem closely allied to that of the movement of the people.

We know that in the opening years of the nineteenth century the bridging of the Thames was, as now, the subject of much controversy, and that it required the genius of a Rennie, a great civil engineer, to design and erect a bridge which has excited the most amazing admiration of the architects of the present century. That bridge has unfortunately in part failed under the stress and strain imposed upon it by the ever-increasing volume and weight of London traffic, and must either be strengthened or give place to another.

In the year 1903 the traffic problem had assumed such proportions, and caused so much inconvenience, congestion and delay, that the aid of Parliament was invoked, and as the result a Royal Commission was appointed to enquire into the question, armed with very wide terms of reference. That Commission, after two years of the most assiduous deliberation and study of the volumes of evidence submitted, made recommendations for improvements to which, unfortunately, effect was never given, but this Report, like that of many another Royal Commission, found its way into the archives of the appropriate Government Department, those responsible for the appointment of the Commission no doubt being satisfied that it had served its purpose by removing from their immediate purview a difficult and troublesome problem.

Subsequently, at least two Departmental Committees were set up to make similar enquiries—the last in 1920, of which I was a member, the Chairman being the late Mr. Kennedy Jones, M.P. The Reports of these Committees, like those of the Royal Commission, were unproductive of results.

In 1921, the London County Council having made representations to Parliament that in its view it was desirable that the London Local Government Area should be enlarged by the absorption of territory from some of the Home Counties, and there being very pronounced opposition from these areas to the proposal, Parliament appointed another Royal Commission, presided over by the late Speaker (the Rt. Hon. Viscount Ullswater), to enquire into the application. It was soon evident that amongst the many involved subjects which engaged the attention of the Commission, probably the one that impressed them as being of the greatest urgency was that of the provision of adequate transport facilities.

The outcome of the recommendations made by the Ullswater Commission was the passing of the London Traffic Act, which received the Royal Assent in August, 1924, and which led to the appointment, under the Minister of Transport, of the London and Home Counties Traffic Advisory Committee, of which I have the honour to be Chairman.

The Committee held their first meeting in January, 1925, and speedily realised that they were face to face with very troublesome matters, which, unless promptly dealt with, might lead to the suspension of many of the necessary transport services then in operation.

Until the passing of the Traffic Act there was no power in anyone to refuse to license omnibuses to ply for hire in the Metropolitan Police District, so long as the vehicles complied with certain structural requirements, and the Chief Commissioner regarded the routes upon which they were to run as reasonably safe.

Prior to the War the London General Omnibus Company and its associated companies had a virtual monopoly of the omnibus traffic within the area, and in 1914 there were 3,664 such vehicles licensed. For service in the War upwards of 1,000 omnibuses were sent overseas to help in the transportation of the troops, the Government when commandeering these giving an assurance to the Company that its interests should not be prejudiced thereby. It was not until April of 1923 that the London General Omnibus Company and its associates had restored the fleet to its 1914 strength.

The first independent omnibus came upon the streets in 1922. In October of 1925 the number of vehicles operated by independent proprietors was no less than 624—101 of these belonging to an equal number of individual owners. At that date (October, 1925) the total number of omnibuses licensed to ply for hire in the Metropolitan Police District was 5,502, an increase of 1,838 as compared with 1914.

The increase in omnibus facilities is even greater than these figures would indicate, as in pre-War years the average seating capacity per vehicle was 34—to-day it is 50. When, therefore, to the increased number is added the increased seating capacity, it will be seen that facilities in this form of transit have considerably more than doubled since the War years.

The omnibus has not only been used to develop new areas, but it has been, and is being, employed to a large extent in direct competition with other forms of transport. This competition has been financially detrimental to the railways, whilst it has been nearly disastrous to the tramways. Almost more serious still is its effect in arresting development in rail and tube construction, much needed now to meet the requirements of the public, who show an increasing tendency to live further and further from the centre. Moreover, such procedure is wasteful and uneconomic, and in the long run the public has to pay.

By Section 7 of the Traffic Act Parliament decided that undue congestion of the streets, and unlimited competition with alternative transport facilities already in existence, must cease. The Committee immediately proceeded to give effect to these mandatory directions. To stabilise the omnibus position they decided to recommend the Minister to declare certain streets as being restricted to the amount of omnibus traffic then upon them, and such restriction orders were in fact applied to no less than 695 streets. This action largely prevented any additional omnibuses being licensed.

Up to the year 1922, with one or two small exceptions, the tramways operating in the area were making quite substantial profits, the publicly-owned undertakings in the year ended the 31st March, 1922, showing profits of £589,493. In the year ended March, 1925, the losses of these municipal undertakings as a whole amounted to nearly £154,000, the London County Council's tramways making a loss of £278,000. In the year now about to terminate it is known that these publicly-owned undertakings will have a deficiency of at least £350,000. The tramways companies have been even more unfortunate than the publicly-owned undertakings. In the case of the Metropolitan Electric Tramways, Limited, the results of working for the year 1925 show, after meeting interest on debentures and loans, rentals, and other fixed charges, a deficiency of £47,242, whilst the corresponding deficiency in the case of the London United Tramways, Limited, amounted to £29,149. The latter company had carried forward a deficit of £16,356 from the previous year, which, added to that for 1925, leaves them with an accumulated deficit of £45,505 at 31st December, 1925.

These deplorable financial results are entirely due to the intensive and wasteful omnibus competition, and as the Traffic Committee are unanimously of opinion that the tramway services are essential to the satisfactory traffic movement of the Metropolis, they are now, and have been for some time, engaged in examining each route, taking statistics of the seating accommodation provided and the extent to which this is utilised: and, having ascertained the facts, they are recommending the Minister to restrict omnibus services so that statutory undertakings may have an opportunity of living, and of earning enough to enable them to pay their way.

The Committee have to meet much badly informed criticism, such as that of their alleged endeavour to protect out-of-date transit methods, thus compelling the public to use a form of transport towards which they are not favourably disposed. Facts are, unfortunately for these critics, absolutely against them.

Railways and tramways are, unlike omnibuses, statutory undertakings, sanctioned by Parliament, and having imposed upon them certain well-defined duties and obligations. One of the most onerous of these obligations is that they shall run regular services, and that during certain defined hours they shall carry work-people at cheap fares which are generally quite unremunerative. Very large numbers of people are in fact so carried, and this at times in the early mornings when few, if any, omnibuses are in evidence. Were it not for these railways and tramways, working people, who have in the majority of cases to travel considerable distances to and from their homes, would not be able to get to their employment at the required time. During these early hours when cheap fares rule the tramcars are in many cases loaded to their utmost capacity, but for the rest of the day, when the tramcar might (and did up to 1922) recoup itself for the loss sustained in fulfilling its statutory obligation, it is met with this intensive and ruinous competition.

Again, it is often thoughtlessly urged that those responsible should recognise the hopelessness of the tram position, and should cut their losses by scrapping their entire undertakings. Any such proposition shows a poor appreciation of the position.

The capital invested in tramways in the area amounts to no less than £27,000,000, of which the London County Council have expended £17,000,000. Of the remaining £10,000,000, £4,640,000 have been expended by other local authorities, and £5,360,000 by private companies.

As compared with the capital invested in tramways, it may be observed that the capital of the London General Omnibus Company, which owns and operates about 75 per cent. of the omnibuses plying for hire in Greater London, is less than £7,000,000, or about one-fourth of that expended upon tramways.

It is, of course, unthinkable that the splendid service given to the multitude by the London County Council Tramways should cease, but if one were called upon to visualise such a disaster in our transport services, the financial effect upon every London ratepayer in the repayment of debt charges would be colossal, and the cost of road repair now charged to the tramway undertakings, and estimated at £300,000 annually, would also be transferred to the rates.

I have already mentioned that the Committee are required to have regard to and supervise an area within a circle extending about 25 miles from Charing Cross. This area takes in the whole of the Counties of London and Middlesex, parts of the Counties of Bucks, Herts, Essex, Kent and Surrey, together with the County Boroughs of West Ham, East Ham and Croydon, with a population estimated in 1924 at 7,679,218.

In the days before the War statistics show that each resident in Greater London travelled 301 times a year, whilst since the War each person would appear to travel at least 450 times a year. It is difficult to account for this, unless it be the result of the War, which caused much unrest, and a desire on the part of the public to be continually on the move. Whatever the cause, the fact remains that this increased amount of travel has accentuated the difficulties of all transport undertakers, as, owing to the alterations in the hours of labour, the rush or peak hours of traffic are now condensed into some 4 or 4½ hours per day, as compared with from 6 to 7 hours at a period when people travelled less frequently.

It may be of interest if I give you the figures showing the number of people carried by each form of public transit in the year ended December, 1924.

By omnibus	1,485,000,000
Suburban Railways	474,000,000
Underground Railways	369,000,000
Tramways	983,000,000
	3,311,000,000

(The number of people carried by omnibus in the year ended December, 1913, was 736,000,000, whilst in the year ended December last it is returned at 1,600,000,000.)

This increased travel by omnibus has been secured at the expense of the statutory undertakings, suburban railways having sustained a loss in the year of 14 million passengers, whilst the tramways were short of no less than 18 millions.

All this travel on the surface of the streets is not only undesirable but is extremely costly to the community. Every additional omnibus adds to the congestion of the streets, and thus assists in causing delay and consequent expense to commercial and business houses, adding to overhead charges, and in this way making it more and more difficult for producers to compete in the markets of the world.

To contemplate a wholesale widening and enlargement of the streets is unthinkable. It would cost untold millions and is quite unnecessary.

The tendency of the worker is to live further and further away from his place of employment, and it is most desirable for hygienic reasons that he should be encouraged to do this. Such encouragement could best be given by the extension of rapid rail transport, either wholly by tube, or by tube and surface railway, but these much desired facilities will not be provided unless the capitalist can be assured of a reasonable return upon the money invested in such ventures. To attract capital, wasteful and unnecessary competition on the surface of the streets must be eliminated. The omnibus has a place, and a very large one, in the future, and so has every other form of transport now available to us; but it is essential that all services shall be made mutually complementary, the chief object being that each shall be self-supporting and at the same time meet the requirements of the travelling public.

If the members of the Committee had ever had any doubts as to the wisdom of the procedure they had decided upon, these would have been dispelled by the facts adduced at a public inquiry held at the end of last year—and extending over ten days—into the alleged inadequate facilities for the transport of people from Finsbury Park to destinations northwards.

Finsbury Park is the terminus of the Electric Underground Railways, the consequence being that each morning and evening large numbers of people have to transfer from bus and tram to rail, and vice versa, filling the streets and adding to the serious congestion which is a chronic condition of that centre. Although buses and tramcars are in service in great numbers, the inconvenience, discomfort, and I fear all too often danger to health of passengers is ever a menace. Upon a wet night in the winter there is a scramble for seats in the road vehicles as they arrive, and it is no uncommon occurrence for the weaker and less active amongst those desiring to travel, to have to wait as long as 35 minutes for the rush period to expend itself before they feel they can safely board a car or a bus.

One would have thought that having all these people to move, and with the absence of serious rail competition, tramways could have earned enough to have enabled them to pay their way; but this is not the case, the reason being, as already stated, the statutory obligation which compels them to carry people at cheap fares, and the intensive omnibus competition they have to meet during the slack hours of the day.

The evidence laid before the Committee at the inquiry by the representatives of Local Governing Authorities, Ratepayers' Associations, and private individuals, was to the effect that all were satisfied that nothing short of an extension of tube railways northwards, and the electrification of the suburban railways of the London and North-Eastern Railway Company, could meet the transit requirements. The Committee have unanimously reported that this is their view also. How is it possible to attract capital for such works unless assurances can be given that road surface competition will be so regulated as to make it reasonably certain that the tubes and railways when in evidence shall have a chance of earning a dividend ?

The stage through which we are now passing in the endeavour to regulate London traffic is by no means unique. Nearly every important city in the United States has either experienced a similar state of things, or is faced with it at the moment. The cities of New York, Boston, Newark, Philadelphia, Detroit, and Chicago have gone through an almost similar period. When I visited these cities to study traffic conditions in the autumn of last year, I went very thoroughly into the matter with the responsible traffic experts and representatives of governing authorities, and obtained first-hand information of their difficulties, and of the methods they had employed, or were employing, in surmounting the same.

The history of the rapid transit and elevated railways and of the street car in the United States is that of our own suburban and electric tube railways and of our tramway undertakings. In at least one city, in consequence of the inaction of the State authorities, the omnibus competition actually led to a shutting down of the tramway or street car undertaking. When this occurred the travelling public and the business people in the city speedily felt the effects, and after suffering the inconvenience and loss for about ten days, it was recognised that the street car did play a very important part in the transport movement and could not be dispensed with. Representations were accordingly made to the Government of the State by the appropriate Local Authorities, with the result that negotiations were at once opened with the owners of the street car undertakings, and promise was given of State protection against unfair competition. The street car service was resumed, and the promise made by the State duly implemented.

The protection clause against wasteful competition is now almost common form in every State. Commissioners of transit control have been appointed, responsible only to the Governor of the State, their duties being to supervise all transport undertakings and to control the licensing of public service vehicles. Before a licensing authority can issue a licence to permit an additional vehicle to ply for hire, it must obtain a certificate from the Board of Commissioners showing that the Board is satisfied that public necessity and convenience would be met by the granting of such licence.

In the City of Boston all forms of transit are now under unified control. The Board of Control appointed by the State of Massachusetts has superseded all authorities and companies hitherto concerned in transport services, and although the identity of each undertaking is maintained, there is common management and a pooling of receipts. The undertakings, many of which were in a parlous state, are now self-supporting, and those who have invested capital are receiving small dividends thereon.

What has been accomplished in the United States can be accomplished in London, and I would ask for the Traffic Committee the confidence and support of a much larger audience than I am addressing now. After all, the problem is not dissimilar to that of a large business which has been badly managed and has got out of hand.

The comfort, necessity and convenience of the residents and ratepayers of this great Metropolis must be paramount, and must always come before private interests, however important. These latter must be treated firmly and equitably, but must always be subservient to the public they are authorised and have undertaken to serve.

I fear I have wearied you with the details dealing with finance and unhealthy competition, but my excuse must be the importance of the subject just now to every Londoner, and the necessity for a clear understanding of the position.

Given a satisfactory settlement of the management and operation of all forms of transit, by which all are put upon a proper financial basis, then I should have no misgivings for the future. I do not mean by this that the traffic problem will be solved. On the contrary, I know that the best staff work and the most assiduous attention must be continuously given by the Committee as a whole if the regrettable delays, which are all too frequent, are to be mitigated, and the causes of congestion removed.

In this huge area we have grave obstructions and delays due to—

1. The all too frequent disturbance of the streets by Local Authorities and Public Utility Undertakings.
2. Street markets, such as the Hay Market at Aldgate.
3. Inadequate means of access to the Docks. Blockage of traffic by railway level-crossing gates and swing bridges over waterways.
4. Weak under-bridges necessitating long and tortuous detours.
5. The all too prevalent practice of parking cars in streets.
6. Cruising taxi-cabs.
7. Insufficient bridges crossing the Thames.
8. Unnecessarily long delays at important street crossings.
9. The absence of alternative routes to the main business streets.

1. By Section 4 of the Traffic Act all Local Authorities in the area are required to submit to the Minister twice in each year statements showing their proposals for road reconstruction and maintenance, and it is the duty of the Minister to examine these proposals, co-ordinating the operations, defining when the works shall be undertaken, and settling dates for commencement and completion.

The Minister and the Traffic Committee are glad of an opportunity of acknowledging the splendid team work which has been manifested by all authorities, and the sporting way in which they have agreed the decisions, even when these have not been quite in accordance with their wishes. The greatest desire to co-operate has been shown, and the time-tables as to dates of commencement and completion have been worked with remarkable accuracy. There is, however, one fatal omission in the clause referred to, which should be remedied at the earliest opportunity. Whilst the Post Office and Local Authorities have been put under the regulations, the undertakers responsible for public utility services and having power to break up the streets are still at large. The system which has been productive of such good team work and good results up to a point should be made sufficiently wide to embrace all authorities and companies having the right to interfere with streets and highways.

2. Street markets are a cause of great obstruction, probably the most conspicuous case being that of the Hay Market in the Whitechapel Road at Aldgate.

The London County Council would have secured the removal of this serious menace to traffic some years ago, but were prevented by some technical legal formalities. Now, however, there is good reason for believing that, with the co-operation of the City Corporation, the Ministry of Transport, and the Traffic Committee, the County Council will see its desires realised. The Minister, in conjunction with the London County Council, has taken the matter in hand, plans and estimates having been prepared for the required alterations, and so soon as Parliamentary powers have been obtained a commencement may be expected.

3. Probably one of the most pronounced causes of delay to all traffic in the London area is the inadequate provision for getting to and from the Docks. For years the necessity for improvements has been recognised, but as it did not appear to be the business of any particular authority no effective action has been taken.

In the days before the War the Port of London Authority urged the necessity for the improvements, and pointed out the great delays and the costs occasioned thereby. The then Road Board agreed to contribute, and the Local Authority in whose area the Docks are situate was not unwilling to assist. The scheme which has been prepared and worked out in detail is, of necessity, a costly one. It involves a new bridge over the River Lea; the demolition of a considerable number of poor-class houses, with the obvious obligation of providing new tenements for the people dispossessed; the construction of a viaduct over the London and North-Eastern Railway; and the erection of a swing bridge over the waterway giving access to the Docks.

There is no doubt as to the urgency of the scheme. Although, as stated, it is an expensive one, yet the cost of the delays occasioned by the present state of things must run into many thousands of pounds a year. On the ground of economy, therefore, the Traffic Committee are satisfied that the scheme should be proceeded with immediately, and have given it priority in a long list of works upon which they have reported to the Minister of Transport.

4. There are many instances where important main highways are seriously reduced in usefulness owing to the presence of inadequate under-bridge accommodation. Most of such structures are the properties of statutory undertakings, and it is realised that there are difficulties in getting improvements effected. The Traffic Committee are, however, considering what action they can usefully recommend to be taken.

5. The question of parking cars in the public streets is causing in London, as elsewhere, considerable trouble and controversy. Whilst the Chief Commissioner of Police and the Minister are desirous of showing the greatest consideration to the owners and drivers of vehicles, still it must be recognised that a public highway cannot, and should not, be considered a substitute for a garage. A street is far too expensive to be used for such purpose, and the proprietors of our large stores, shops, and hotels should be induced to make arrangements for providing suitable accommodation in positions other than on the highways for their customers' cars. I am glad to say some firms have already done this, whilst others are negotiating for accommodation.

As the numbers of motor vehicles in the country are increasing at the rate of about 200,000 a year, if congestion is to be relieved then parking in streets will have to be sensibly curtailed, and people must recognise that, with the greatest desire to be considerate, neither the Commissioner nor the Minister can resist the pressure of public opinion, which will generally be against tolerating obstruction so caused.

The erection of larger and higher buildings, and the greater concentration of people therein, either for employment or residence, must tend to increased congestion, and it should be a requisition of the authorities controlling the construction of such buildings that certain land on the site, or contiguous thereto, would be reserved for the parking of cars, and/or for the loading and unloading of vehicles.

6. The cruising taxi is a difficulty, and is a contributory cause of congestion. The absence of sufficient recognised cab-stands is to some extent responsible, but this question is closely allied to the

subject of parking of cars and the unnecessarily long waiting of private vehicles in front of business premises. If business houses and their customers would assist the Commissioner by diminishing these waiting periods to the smallest minimum, the task of controlling the cruising taxi-cab would be simplified.

7. The Traffic Committee, with their excellent engineering staff, have given much attention to the general bridge question. Statistics of traffic have been taken and an analysis made of its nature, and observations have been made as to the origin and destination of this traffic.

There is no doubt that additional bridges are needed, not because the present ones are unduly taxed as to volume, but in order to minimise unnecessary indirect travel by vehicles, and the congestion and delays occasioned thereby. The width of the bridge and its carriageway, so long as proportions are well balanced, is not of so much importance as that of ample space in, and directness of, its approaches.

A comprehensive report upon the bridge question in the traffic area is being prepared, and in due course it will be presented with appropriate recommendations to the Minister.

8. An examination has been made of the traffic position at most of the important points of intersection in the central areas, and the causes of the congestion and delay which frequently occur have been scientifically studied. There is no one method of improvement applicable to all places, each point of intersection needing to be dealt with specifically.

The procedure of the Committee is first of all to take statistics of traffic, recording the intensity of the several streams, and the particular types of vehicles in each stream. Possessing this information, by taking the average number in each stream over a sufficiently long period we get to know the volume to be dealt with in the several directions. Then we see what road space is available and how this can best be utilised. It is thus we ascertain whether or not it is practicable to have what is called gyratory or continuous working, such as the arrangement now in operation at the end of Parliament Street, Bridge Street, etc., which is stated to be a success, and which certainly succeeds in keeping traffic on the move. A similar system is in operation near the Victoria Memorial, whilst schemes of a more or less identical character have been approved for Trafalgar Square and Hyde Park Corner. Other centres will be taken in hand in order of importance, the Committee being persuaded that considerable improvement in the movement and speed of traffic can be effected in this way by intelligent study and manipulation.

The Committee have also considered the advisability of adopting a signalling system, upon the lines of those in operation with more or less success, in the United States. A commencement with signals is being made in Piccadilly between the Berkeley Hotel and the Circus, and it is hoped this experiment may be in working order in the course of the next few weeks.

9. The Committee have had careful surveys made and reports submitted with a view of ascertaining how far it is possible to arrange for alternate routes for traffic, in order that relief may be afforded to the most intensively used streets in the shopping centres.

In these—e.g. Regent Street, Oxford Street, Bond Street, etc.—it is essential to meet the requirements of the public that passenger vehicles should be largely in evidence, but it will be agreed that through traffic having no business to transact in these streets should be encouraged to take alternative routes.

For instance, a railway van is desirous of travelling from Paddington to the Docks in the East, or a motor car is desirous of getting from the Harrow or Wembley district into the Lea Valley en route to Essex. By utilising many of the existing subsidiary roads which, with minor alterations, may be made continuous and through routes, and with proper and effective direction signs by day and night, it is believed that with much advantage a large amount of traffic can be deflected.

Several complete schemes of this nature have already been formulated and recommendations made thereon to the Minister, who has been pleased to accept and approve the same. Negotiations are now proceeding with the appropriate local authorities, and the Committee are hopeful that they will be successful in their endeavours in segregating the through traffic from the local.

The time at my disposal, and regard for the patience you have already so kindly extended to me, make it impossible in one lecture to do justice to so vast a problem, the satisfactory solution of which is of so much importance to the comfort, health and general welfare of the citizens of the great community we know as London, but which in fact represents nearly one-fifth of the total population of the United Kingdom.

[H. P. M.]

WEEKLY EVENING MEETING,

Friday, January 22, 1926.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., in the Chair.

SIR WILLIAM BRAGG, K.B.E. D.Sc. F.R.S.,
Fullerian Professor of Chemistry.

The Work of the Davy Faraday Research Laboratory.

[ABSTRACT.]

IN this discourse an account was given of the relations of the Davy Faraday Laboratory to the senior Institution. The building and equipment had been presented to the Royal Institution by the late Dr. Ludwig Mond in 1896. They were sufficient for the accommodation of about fifteen workers, and full use was being made of them.

The workers were maintained in one or two cases by the funds of the Laboratory, but these were insufficient to do much in this direction. Many workers held fellowships or scholarships granted by outside bodies, and some were private assistants to the director, who was in receipt of a personal grant for that purpose from the Department of Scientific and Industrial Research. Others again relied on their own resources. The income of the Laboratory itself was sufficient only, with the small exception already mentioned, to provide accommodation, equipment, light, heat, and power. The researches were mainly concerned with the development of the new methods of crystal analysis, especially as applied to organic chemistry, and to some metallurgical problems. The work done was actually the present contribution of the Royal Institution to research in science.

The discourse went on to give some description of the main problems under investigation, but, as the results when obtained have been fully described from time to time in scientific journals, it will be sufficient here to give the following list of recently published papers :—

By W. T. ASTBURY.

"Crystallography" (Ann. Report, Chem. Soc., 1925, vol. 22).

"The Structure and Isotrimorphism of the Tervalent Metallic Acetylacetones" (Proc. Roy. Soc., A, vol. 112, 1926).

"Crystal Structure and Chemical Constitution of Basic Beryllium Acetate and its Homologues" (*in conjunction with* PROFESSOR G. T. MORGAN, F.R.S.) (Proc. Roy. Soc., A, vol. 112, 1926).

By J. D. BERNAL.

"On the Interpretation of X-ray Single Crystal Rotation Photographs" (Proc. Roy. Soc., vol. 113, 1926).

By W. G. BURGERS.

"An X-ray Examination of i-Erythritol" (Phil. Mag., vol. 1, Feb. 1926).

By W. A. CASPARI.

"Crystal Structure of Catechol" (Journ. Chem. Soc., March, 1926).

By W. H. BRAGG and R. E. GIBBS.

"The Structure of α and β Quartz" (Proc. Roy. Soc., A, vol. 109, 1925).

By R. E. GIBBS.

"The Structure of α Quartz" (Proc. Roy. Soc., A, vol. 110, 1925).

"The Structure of Tridymite" (Proc. Roy. Soc. In the press).

By I. ELLIE KNAGGS.

"The Crystalline Structure of Pentaerythritol Tetranitrate" (Min. Mag., vol. 20, No. 109, 1925).

By A. MÜLLER.

"Imperfect Crystallisation in certain Long-Chain Compounds" (Letter to "Nature," May 22, 1926).

By G. SHEARER.

"On the Distribution of Intensity in the X-ray Spectra of Certain Long-Chain Organic Compounds" (Proc. Roy. Soc., A, vol. 108, 1925).

"Orientation in Solids." Contribution to General Discussion on Physical Phenomena at Interfaces, with Special References to Molecular Structure (Journ. Faraday Society. In the press.)

By KATHLEEN YARDLEY.

"An X-ray Examination of Iodo-Succinimide" (Proc. Roy. Soc., A, vol. 108, 1925).

"An X-ray Examination of Maleic and Fumaric Acid" (Journ. Chem. Soc., vol. 127, 1925).

"Three Examples illustrating X-ray Methods of Supplementing and Correcting Crystallographic Data" (Phil. Mag., vol. 1, Oct. 1925).

[W. H. B.]

WEEKLY EVENING MEETING,

Friday, March 12, 1926.

Sir Edward Pollock, F.R.C.S., Vice-President, in the Chair.

Sir J. J. Thomson, O.M. LL.D. D.Sc. F.R.S. M.R.I.,
Master of Trinity.

Radiation from Electric Discharges.

[Abstract Deferred.]

WEEKLY EVENING MEETING,

Friday, March 19, 1926.

Sir Arthur Keith, M.D. LL.D. F.R.S., Secretary and Vice-President, in the Chair.

John Tweed, Sculptor.

Chips from a Sculptor's Studio.

In accepting the kind invitation of the Managers of the Royal Institution to lecture here I feel the honour deeply, but now that I realise what I have to do I am too conscious of the frightening ordeal itself. The Council might have billed this as my first appearance on any stage as a lecturer, and at the moment I can honestly say that it is likely to be the only performance.

It is with hesitation that I venture to speak on the silent art of sculpture, for though it has been my daily work and study, and has given me much joy in pursuing it, I need not add it has given me much misery, for no artist ever realised his dream of the finished work. Yet I find it very hard to talk about—to find the words that will convey what I want to express in another medium. One can discern though not describe.

Artists never work under the tyranny of an eight-hour day, and take advantage of their liberty to work at all times and in all places. Indeed at times one fancies one is losing the power of expressing one's thoughts in words. Indoors and out of doors one is constantly seeking suggestions of movement and pose, and considering the effects of light and shade playing on the features, and how to put them in some plastic medium. Though the ceaseless effort goes on with or without the materials at hand the mind is thinking in terms of clay or marble, and with no thought of interpreting these ideas in words, or any effort to describe the process.

When you come to see a great work of sculpture for the first time there is a delight of greater or lesser intensity; it fills you with joy, and only later do you look at it from the student's point of view, to find how the technical part has been obtained and the treatment of the material used, drawing, composition and design, and the other qualities that are inseparable to a great work of sculpture. The greatest works, like the greatest things in life, have something too often beyond analysis.

It is now, more than ever before, a source of amazement to me that writers who have no practical knowledge of the art are able to write pages and volumes with apparent facility and assurance. When I have occasionally made the effort to read their writings I confess too often to have found nothing but complete confusion of ideas and an irresponsible use of terms. I fancy that the writer gets tied up in trying to explain himself, and flounders deeper as he labours to sustain his views. The writer may produce amazing descriptions and suggest subtle theories about sculpture, good in his own art of writing, but he cannot add to or lessen what the sculptor put there. He often puts up an effective smoke screen.

The great masterpieces, complete or fragmentary, still remain and show us the beauty and the thought carved by the sculptor with love and joy.

At no time has so much been written and spoken about art as during these late years, and yet there was never greater chaos or uncertainty amongst artists and those who consider themselves students or lovers of art. We can discourse on art, but have we lost the creative power? The mere purchasing of works of art does not denote appreciation. There is no standard of perfection, no certain knowledge of material, and often the crudeness of execution is accepted as the finest expression in modern work. It is surely a sign of want of originality to try and invent a new shudder instead of seeing beauty in healthy and living nature.

I should like to record the views of Rodin as he gave them to me one day on the worth of the literary man's appreciation of the plastic arts. It happened that Anatole France came to see the portrait of Clemenceau that he had just modelled, and for the novelist's inspection he had arrayed a half dozen states of the bust. I cannot remember what France said, at the time his remarks struck me as being quite irrelevant. Later I asked Rodin if France knew anything about the art. The old man turned to me with a surprised look that I should have asked the question, and said, "No; no literary man knows anything about the plastic arts, they could never value any fresh expression of the painter or the sculptor"; "though France," he said, "was better because he was willing to learn." Yet the critics write, and we flatter them by exhibiting our works for the practice of their pen, and are surprised that the public should so willingly accept their criticism. We are naturally too indolent to make much effort to reason or discriminate for ourselves, especially on a subject that demands a trained sense of technique, so it is easier and less troublesome to accept our knowledge at second hand. Journalists sometimes bring life to a modern work that should never have been born. Instead of approaching a work of art and discovering what pleasure it holds for us, we prefer to read some critique and then go to the picture or statue flattered that we can see there what we read of. The Chinese have a saying that we see pictures through our ears. I have often been struck with the seeming contentedness of the blind, they appear happy and seem to miss nothing.

The deaf person, on the other hand, usually has discontentment and moroseness as his habitual expression; he misses an easy way of gaining his knowledge. The sense of sight is rarely trained, or the gift of thinking about what we see; hearing we are happy in exchanging ideas and ordinary gossip. It cannot be a creative age when so many critics are dissecting.

I have thought it might be of some interest to you to tell you how a statue is made; I am often asked the question by visitors to my studio. One client evidently had no idea how a portrait bust was done, as on entering the studio his first remark was, "Is it painful?" I tried to assure him that it need not be painful, but that it might be boring to sit. "But," he said, "when you take the plaster off my beard won't it hurt?"

When a work is commanded for a given site the size and material have to be decided on; if it is for part of a building most likely the stone for the building will be selected for the sculpture. If it is to be placed out of doors in this country, bronze is the most suitable. No stone can long retain the delicate work left by the carver. The material chosen for the work controls the design; one has to think of the final material to be used, bronze or stone, clay being only a means of getting there.

In modelling you start, as in painting, from nothing; the painter has his bare canvas, you have a board, and like the painter you go on adding with some soft substance, such as clay or wax, until you have realised in form your motive or idea. As a rule one uses a soft material to work out the original idea. Clay is the easiest and simplest to manipulate; the fingers are able quickly to shape it as the brain directs. Steel or wooden tools can be used, but the human touch has more sympathy and the result gives a less mechanical effect. In doing a bust, statue or group, some support is needed to keep the clay from falling down. In colossal statues, iron and lead are used for the armature, which is like a skeleton. The clay sketch should have life in every touch; it is the most direct plastic expression of the artist's thought, the mind through the fingers controlling the material. The sketch is the birth of a thought; your work later is to develop it, and with patience and toil make it a perfect thing. The labour is arduous and long to achieve this, at the same time retaining the life that the clay model had. Balzac, in "La Cousine Bette," gives an amazing description of this development in an analogy of the birth and training of a child.

Unfortunately the clay model cannot remain permanently, so in order to preserve it, but with a very definite loss of its vividnesss, it has to go through other processes. Some scientist might suggest a means of fixing the clay permanently, at the same time retaining its shape and size, without losing the actual touches of the artist. The clay can be baked, giving what is called "terra cotta," and the result is charming; but you may lose all in the firing, and often the shrinkage is so irregular that it destroys all recognition. Plaster is the usual medium to preserve the work. In this process plaster of Paris is mixed with water in a bowl and flicked on the clay until it is all covered, and the mould or negative is then cleaned or prepared ready for the pouring in of the plaster of the cast; this should fill up and occupy every part of the mould. It soon gets hard, and you proceed with a chisel and mallet to break the mould and detach it from the plaster copy that emerges and gives a replica of your clay model.

If a more permanent material is asked for, bronze or any other metal may be used. Gold is another metal that may be used in sculpture, but it is seldom asked for nowadays. An amazing achievement in burnished gold and precious stones is to be seen on the coffin of Tutankhamen, which was done by gold plates beaten over a hard core. When bronze is required the plaster model is sent to the bronze caster, and he either casts it in the sand or "lost wax" process. This process takes several months. You have a picturesque description of casting in the "ciré perdu" process in Benvenuto Cellini's autobiography. In the old days the bronze founder was an artist, and took a delight in working the surface and quality of his bronzes. Now it is a more or less commercial undertaking, and few artists take much interest in the result.

A cast need not be judged by its weight as is so often done; indeed by reducing the thickness of the bronze a better result is obtained. One has heard of a committee choosing their sculptor from his promise to give a weightier cast than the other competitors. The only care now taken in bronze casting is in the making of the forgery, which is often hard to detect. With care and attention a bronze cast can be repeated from the original model without in any way lessening the value of it as a work of art. Rarity does not make a work of art, it only arranges its sale price.

Then you have carving or sculpture proper, in such materials as stone, marble, granite, or obsidian. Let us start on a marble statue with a block from the quarry, irregular in shape but large enough to take in the model to be transferred into the stone; "you take away the superfluous marble" is the answer as to how a marble statue is made. These materials are worked either by tempered metal tools, or wheeled with some grinding powder. The usual way nowadays is to send your plaster model to a pointer, who, with the aid of a specially designed machine, takes his measurements from the plaster to the marble, or in the older way, still used on the Continent, three points are placed apart at different angles on the plaster model and on the marble similarly, and from these three given points with the aid of three compasses a fourth is found, and so on until the surface of the plaster and the marble are covered with little point marks. Then the carver cuts it still nearer the size of the original so that the sculptor, if he has had any training in carving, can generally finish it himself. Unfortunately, so little marble work is done in

England that we have very few qualified workers, and have to depend on the foreign workers who have more training.

You can see marble with the points left on in old Egyptian, Greek and Italian works. The Egyptian granite statues appear at first sight to have been cut out by a saw. I shall show you later some slides demonstrating the different stages of Egyptian sculpture.

A finer expression and use of marble is to take a block and carve it straight away, the shape of the rough quarried piece often determining the form of the finished statue. The difficulties of doing it this way are not so great as one would think, though the labour of working with a hammer weighing from 3 to 5 lbs. and the hardness of the stone make it no light task.

The texture of marble and its transparency is more like the human flesh than any other medium used. An artist delights to employ it to interpret the human form. If one goes to the Serpentine during the summer months and watches the children bathing, in what sunlight we get there, one can realise there the fullness and simplicity of form seen in sunlight, and so well realised in Greek sculpture. It is not merely an anatomical study of form but of life. A Greek sculptor would have delighted in these figures, and if asked what material he would use to portray this subject he would at once have said marble. Of course the climatic influence enters into our selection of a medium, and a British artist watching this scene in Hyde Park would decide on colour as the medium, or he might consider black and white as the more appropriate. Someone has said that "clay is the life, plaster the death, and marble the resurrection."

In choosing marble or stone as a final material, one considers its brittle nature and that the work, for the sake of preserving its completeness, should as much as possible keep to a solid block form, with no thin projecting parts liable to be broken. A marble statue should be so compact as to be able to be rolled down a hill without any of it breaking. From the nature of bronze you are almost unlimited in the shapes you can employ, points sticking out, limbs, hair or drapery flying, and as thin as you like.

Our only knowledge of the work of the Greek sculptors who specialised in bronze we gain from marble copies, as only a few ancient bronzes of a large size exist.

Bronze is a dark material, and its silhouette has to be considered, dark against the light colour of the sky. Bronzes can be melted down for other uses, as in a period of destruction like the late war, when metal objects were taken for guns, &c.

We have described the ways and means of the sculptor's work, and now I propose to consider why and how he uses them. This is not an easy work for the practical sculptor. He works with these materials to realise an idea inspired by the beauty of the forms observed in nature. His brain is directing his hand in the interpretation of the living form into the lifeless clay, with a result to be felt rather than described. To the artist it seems foolish and useless to make the effort of putting it into words, and I do not think it is possible to convey to another, who has not had similar experience of the technical side, all that one is striving for. Another great difficulty that hinders clarity of explanation is the terms employed; the arrangement of planes and the balance of form. These are often used with different meanings. With the scientist it is different; talking of his doings he employs words that carry with their use a definite meaning and significance. Art, a word constantly being used to designate vaguely pictures, sculpture, music, and sometimes architecture, always includes temperament. The artist, like the scientist, gets his inspiration from the study of nature. The scientist examines it in his search for living organisms. The artist accepts his theme from nature, but mentally frees and creates a thing absolute in itself. Thus the work of art lives apart and irrespective of any object. It has no educative nor moral reason for its existence, it depicts the perceived beauty in natural objects.

Sculpture is the first sign of man's civilisation. Primitive super-man maintained his superiority over another by giving the knock-out blow; but later came the need or wish of the super-man to respect and reverence some higher power, and embody it in some form to worship. So the potter, that had so far shaped the pots, or the carver who carved the daily utensils, was called in and ordered to make some symbol or image that the super-person could bow to and worship. It may have been a crude affair at first, but they advanced in their power of imitating the figures of people. The first sculptors thought that if they could make a model of the best-grown man around, then they would realise the idea asked for by their chief. The sculptor had a good start: the rulers, kings or priests commanded the works to be done, and a place in the temple was assured for their reception. We have got to a theme that has been the aim of the sculptor, to portray the form of man divine. Now he starts on his job. At first wood was the material used; the figure was portrayed standing like a column with the arms and legs close together. From the first it was never an art of literal imitation; no art can be that—if we think for a moment we must realise that the view is absurd. Let us take the human figure and consider what it is composed of. We need only take note of what is superficially seen; bones, muscles, tendons, skin and hair, you have these varied parts in constant movement, altering the form and colour; these changes have to be interpreted in one medium. Let us take a hand: you can have a cast taken of it in the same process that I have already described for the clay model; this will give you an actual facsimile in plaster of the living hand. How does it impress you? As a dead white model of a seeming lifeless object. The sculptor takes the same object as his study, and tries to interpret his impression of it; he finds that the fleshy parts have roundness and fullness of shape, one part reflecting against the other seems to make them larger than their mechanical measurements, so the compass will not help him ; he finds the skin over the bones alters colour from the general colour, so he must increase the salient parts and lessen the depths of the hollows. I have tried to describe something of the procedure of the sculptor in his effort to give a suggestion of a living object. The better result will be gained the more he departs from mere imitation. Something of this may be communicated to the student, but the keenness of perception, the subtle gift of discrimination, the inexplicable inspiration that goes to the making of a work of art, these cannot be taught. No industry or intellectual devotion can make the artist, that indefinable power is not communicable. One must learn from experience to know, analyse, eliminate and later create, which is knowledge beyond knowledge. One constantly finds the most beautiful workmanship without that quality ; it is of smaller value to the world than the less accomplished work with that one touch of inspiration. An inspired work is as rare as the gem, but it is as worth searching for. Art is dependent on the individual, in fact it is individual expression of beauty in nature.

The mechanical part of sculpture can be taught as we have already seen. In the past it was so well done that it is often difficult to detect the hand of the pupil from that of the master. In the Parthenon frieze, where many pupils must have worked, you can detect very unequal treatment. Phidias, as the master sculptor, must have trained his band of pupils to do his bidding well, and they were willing to follow his teachings slavishly and with great industry.

It may be better understood if I show how art differs from science. In science the accumulated facts make for progress ; the student of to-day is far in advance of the pioneer of his special branch, owing to the added knowledge and the development of fresh instruments. Leaving out the absurdities of extreme sculpture we cannot say that art has progressed from where Phidias and Praxiteles left off.

We will try to find out what we can obtain from studying two schools of the past, the Egyptians and the Greeks. One can visit our museums, where have been collected a varied assortment of specimens, exposed as in a sale room, without arrangement or taste, but you cannot realise the beauty and fitness of the art without seeing them in their place, and under the climatic conditions that controlled and aided the artist. Curators are rarely artists, and so they collect for historic reasons, and find their pleasure in the rare and curious, seldom seeking for the beautiful.

Egypt, a land of about 600 miles in length, with an average width of seven miles, bounded by the limestone hills on either side of the Nile ; the sun warm and brilliant, but not giving clarity of shape to the hills, nor tending to define the temples built there of sandstone or granite with their long lines fitting into the landscape; the extraordinary peace of the desert, the long horizontal lines not as a rule bounded by high hills, and all that soft warm colour is a fitting background for the great heavy temples. The sculpture of the Egyptians was used for the glorification of death and the beautifying of tombs. Strangely familiar is their depicting of the common life, in some periods with great beauty and accomplishment, and the treatment of their relief work. In some of their wooden figures there exists an extraordinary realism, also in their granite and harder materials, simplifying almost to emptiness of form. I mean that material controlled their treatment.

All that Europe has taken to Egypt is discord and dams; all that Europe has taken from Egypt is the idea of the dreary dull khaki colour.

With the Greeks sunshine made the carved piece a crystal to the opal effect in Egypt. The greatest thing for admiration in all their art is a sense of proportion; they have been our teachers and given us a lead in all branches of thought, but they seemed indifferent to become rulers. With no understanding of the Hellenic life or thought, admiration and appreciation of Greek art from the Christian era until the present day has always existed, together with a feeling of intimate relationship. In sculpture the Greeks depicted their gods in perfect human forms, they did not deal in symbols. Art was only one of their many forms of culture.

At the moment it is the habit of the critic and some writers to decry the great beauty and power of the finest Greek sculpture. The critic puts negro work in its place; I don't doubt their understanding in preferring it, and with the sculptor it may lessen his difficulties in achievement; every age and country has the art it deserves.

[J. T.]

WEEKLY EVENING MEETING,

Friday, March 26, 1926.

The Hon. Sir Charles A. Parsons, K.C.B. Sc.D. LL.D. F.R.S.,
Vice-President, in the Chair.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. F.R.S. M.R.I.,
Professor of Natural Philosophy R.I.

The Radiation from Atomic Nuclei.

[Abstract Deferred.]

GENERAL MONTHLY MEETING,

Monday, April 12, 1926.

SIR EDWARD POLLOCK, F.R.C.S., Manager and Vice-President, in the Chair.

H. S. Broom,
Mrs. Lionel Bulteel,
Mrs. Charlesworth,
Miss E. M. Delf, D.Sc.,
Mrs. G. H. Moore-Browne,
Samuel Barnett Schryver, D.Sc. Ph.D.,

were elected Members.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., etc., was nominated for Election as Honorary Professor of Natural Philosophy at the next General Meeting on Monday, May 3, 1926, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. F.R.S., etc., was nominated for Election as Professor of Natural Philosophy at the next General Meeting on Monday, May 3, 1926, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

The following Lecture Arrangements AFTER EASTER were announced :—

JOSEPH BARCROFT, Esq., C.B.E. F.R.S., Fellow of King's College, Cambridge, Fullerian Prof. of Physiology. Four Lectures on ORGANS OF MULTIPLE FUNCTION: 1. THE SALIVARY GLANDS; 2. THE SPLEEN; 3. FEATHERS; 4. LUNGS. On *Tuesday*, April 13, *Wednesday*, April 21, *Tuesdays*, April 27, May 4.

G. W. C. KAYE, Esq., O.B.E. M.A. D.Sc. M.R.I., Superintendent of the Physics Department, The National Physical Laboratory. Three Lectures on THE ACOUSTICS OF PUBLIC BUILDINGS. On *Tuesdays*, May 11, 18, 25. (The Tyndall Lectures.)

BRIGADIER-GENERAL SIR PERCY SYKES, K.C.I.E. C.B. C.M.G. Two Lectures on: 1. SHAH ABBAS OF PERSIA, THE CONTEMPORARY OF QUEEN ELIZABETH; 2. CHINESE TURKESTAN AND THE PAMIRS. On *Tuesdays*, June 1, 8.

OLIVER H. P. PRIOR, Esq., M.A. D. ès L., Fellow of St. John's College, Cambridge, Drapers Prof. of French, University of Cambridge. Two Lectures on: 1. FRENCH RHYTHM AND ENGLISH POETS IN NORMAN AND MODERN TIMES; 2. ANGLO-NORMAN LITERATURE: AN INTRODUCTION. On *Thursdays*, April 15, 22.

Sir William Bragg, K.B.E. LL.D. F.R.S. M.R.I., Fullerian Prof. of Chemistry. Three Lectures on The Imperfect Crystallisation of Common Things. On *Thursdays*, April 29, May 6, 13.

Ulick R. Evans, Esq., M.A. Two Lectures on Corrosion, Tarnishing and Tinting of Metals. On *Thursdays*, May 20, 27.

J. Newton Friend, Esq., D.Sc. Ph.D. F.I.C., Municipal Technical School, Birmingham. Two Lectures on: 1. Iron in Antiquity; 2. Science in Antiquity. On *Thursdays*, June 3, 10.

William T. Calman, Esq., D.Sc. F.R.S., Deputy Keeper of Zoology, British Museum. Two Lectures on The Shipworm. On *Saturdays*, April 17, 24.

Percy C. Buck, Esq., M.A. Mus.Doc. M.R.I., Director of Music in Harrow School. Two Lectures on The Song Form in England as represented by (1) Stanford, (2) Parry. (With Musical Illustrations.) On *Saturdays*, May 1, 8.

George C. Simpson, Esq., D.Sc. F.R.S. M.R.I., Director of the Meteorological Office. Two Lectures on Atmospheric Electricity. *Saturdays*, May 15, 22.

M. Alexandre Moret, Professeur au Collège de France, Paris. One Lecture on Une Révolution Sociale en Egypte vers 2000 Av. J-C. *Saturday*, May 29.

Sir Walford Davies, Mus.Doc. LL.D. One Lecture on The Triad and the Perfect Fourth; 2. Their Uses from Hucbald to the Present Day. (With Musical Illustrations.) On *Saturday*, June 5. (Postponed from Before Easter.)

Howard Carter, Esq., LL.D. One Lecture on The Examination of the Mummy of Tut-Ankh-Amen. On *Saturday*, June 12.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

Athenæum Club (the Committee)—History of the Athenæum, 1824-1924. By Humphry Ward. 8vo. 1926.

Boyle, Miss M. E. (the Author)—Barma Grande—The Great Cave, and its Inhabitants. 8vo. 1925.

Browne, C. A. (the Author)—The Life and Chemical Services of Frederick Accum. 8vo. 1925.

German Government—Wissenschaftliche Ergebnisse der Expedition Filchner nach China und Tibet, 1903-1905. Bands 2, 3, 4, 5, 6, 9, 10 and 11, and Maps. 8vo. and fol. 1908-1914.

Hydro-Electric Power Commission of Ontario—Engineering Report on Proposal to develop the St. Lawrence River. 8vo. 1925.

Parker, W. Rushton, Esq., M.D. M.R.I.—
Æsop's Fables. 2 vols. 8vo. 1793.
Anderson, J. C. Shropshire. 8vo. 1864.
Buchanan, G. History of Scotland. 4 vols. 8vo. 1827.
Bunyan, J. Pilgrim's Progress. Illustrated by J. C. Selous. Obl. 4to. 1844.
Disraeli, B. Miscellanies of Literature. 8vo. 1840.
Fénelon. Adventures of Telemachus. 2 vols. in one. 12mo. 1809.
Gay's Fables. 2 vols. 8vo. 1793.
Goldsmith, O. Animated Nature. 2 vols. 8vo. 1852.
Green, H. Shakespeare and the Emblem Writers. 4to. 1870.
Harland, J., and Wilkinson, T. T. Lancashire Folklore. 8vo. 1857.
Hood, Thomas. Works. 11 vols. 8vo. N.D.
Land of the Rupee. 4to. 1912.
Lawrence, H. Queens of England. 2 vols. 8vo. 1838-1841.

Maspero, G. Ancient Egypt. 8vo. 1892.
Master Humphrey's Clock. Vols. I.–III. 8vo. 1840-1841.
Menpes, G. Japan. 8vo. 1903.
Morley, H. First Sketch of English Literature. 8vo. 1889.
Napoleon. Memoirs of the History of France; dictated to General Gourgaud. 2 vols. 8vo. 1823.
Parkyn, G. J. Monastic Remains. 2 vols. in one. 8vo. 1816.
Pennant, T. Some Account of London, Westminster and Southwark. 2 vols. 4to. N.D. (circ. 1826–30).
Prichard, J. C. Egyptian Mythology. 8vo. 1819.
Rimer, W. Illustrations to Thomson's "Castle of Indolence." Obl. 4to. 1845.
Scott, Sir W. Border Antiquities. 2 vols. 4to. 1814–1817.
Stanley, A. P. Sinai and Palestine. 8vo. 1862.
Strutt, J. Sports and Pastimes of England. 2nd edition. 4to. 1810.
Tour of Dr. Syntax in Search of Consolation. (Combe, W.) Illustrated by Rowlandson. Vol. II. only. 8vo. 1820.
Tour of Dr. Syntax in Search of the Picturesque. (Combe, W.) 8vo. 1838.
Tytler, S. Tudor Queens. 8vo. 1896.
Universal Songster. 3 vols. 8vo. N.D.
Weekley, E. Concise Etymological Dictionary. 8vo. 1924.
Wheeler, W. A. Familiar Allusions. 8vo. 1882.
Whitney, G. Choice of Emblemes. Edited by H. Green. (Facsimile Reprint.) 4to. 1810.

WEEKLY EVENING MEETING,

Friday, April 16, 1926.

Sir James Crichton-Browne, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Arthur W. Hill, C.M.G. Sc.D., F.R.S.,
Director of the Royal Botanic Gardens, Kew.

The Quest for Economic Plants.

Several of the economic plants which are of great value at the present day were transported about the world in prehistoric times, while the introduction of others has taken place in recent times, and the history of the introduction is well known. Many interesting and vital problems connected with all plants grown for economic purposes still await solution, and an allusion to some of these problems is made at the end of the discourse.

With regard to plants the native countries of which are somewhat uncertain, the coconut, banana, sugar cane, ground nut, and the oil palm may be mentioned.

With regard to the coconut, some authorities hold the view that it originated in America, and was disseminated westwards through the Pacific to Malaya, while others maintain that it originated either in the Pacific region or the East. The arguments put forward by the late Professor Beccari that the Polynesian or Asiatic region was probably the original home of the coconut seem convincing.

The banana is another economic plant now found all over the world, but no wild specimens are known from the American continent. An interesting discovery has recently been made of fossil banana seeds in Tertiary rocks in Colombia, South America, but these are closely related to the non-edible species (*Musa Ensete*), from tropical Africa, and probably have no connection with the edible banana. Though the edible banana is found extensively on the American continent at the present time, it seems unlikely that it originated there, and that tropical Asia or the neighbourhood of Indo-Malaya is probably the original home of the species from which the edible banana has been evolved.

Another interesting plant whose original home is somewhat of a problem is the West African ground nut (*Arachis hypogea*), which is now the staple crop of the colony of the Gambia. From the evidence available it seems probable that the original home of this economic plant is South America, and that it was introduced to West Africa by early voyagers across the Atlantic Ocean before we have any record.

The original home of the sugar cane is also somewhat obscure. It was certainly introduced to the West Indies, and it seems probable that it was carried westwards from its original home, which seems to have been the East Indies.

The oil palm of West Africa also offers a problem, as it has sometimes been thought that its original home was South America, since there is a closely allied species apparently native in Brazil. Recent research, however, has shown that an allied species occurs in Madagascar, and it may well be that the original home of *Elæis guineensis* is really tropical West Africa.

Many other plants of economic importance, such as those which yield various cereals and drugs, offer somewhat similar problems as regards their native home, but it is economic plants inhabiting tropical regions which have been introduced in historical times, that I wish to bring to your notice.

The classical case of one of the earliest historical attempts in plant introduction is the ill-fated voyage of the "Bounty," an enterprise fitted out with the object of bringing the bread fruit (*Artocarpus*), from the Pacific Islands to the West Indies. This enterprise was set on foot under the auspices of Sir Joseph Banks, who was in effect the first Director of Kew, and in that capacity was botanical adviser to George III., from the year 1772 until 1820, the year in which both Sir Joseph and George III. died.

The voyage of the "Bounty" (Dec. 23, 1787), is one of special interest to Kew, since Sir Joseph Banks selected a Kew gardener, David Nelson, to accompany Captain Bligh to look after the young *Artocarpus* plants which were to be conveyed to the West Indies. In the narrative of the voyage a detailed plan is given of the arrangements for the storing and packing of the pots of plants on the decks. This enterprise was, as far as I know, the first definite attempt to introduce an economic plant on a large scale from one part of the world to another. As is well known the undertaking was a failure, owing to the mutiny on April 28, 1789; but later the bread fruit was successfully taken to St. Vincent by Captain Bligh in the year 1793 in the "Providence."

The romance of the quest for economic plants has been somewhat curtailed and the methods greatly simplified by the invention of Nathaniel Bagshaw Ward, Examiner to the Society of Apothecaries from 1836–1854. It was owing to his invention of Wardian cases, which are in effect miniature green-houses, that the introduction of tea from China to India in 1848 by Robert Fortune, Curator of the Chelsea Physic Garden, was rendered possible.

This was followed by the introduction of *Cinchona*, the source of quinine, from the Andes of Northern South America to Kew by Sir Clements Markham in 1861, and thence to India; and by the introduction of Para rubber (*Hevea brasiliensis*), in 1876 by Mr. (now Sir Henry) Wickham, Mr. Cross, and others from Brazil through Kew to the East.

Cocoa, coffee, tea, cloves, nutmegs, camphor, cotton, limes, sisal hemp, varieties of bananas, and many other useful plants have been transported by these means, not only to our own colonial possessions, but to all parts of the tropics. The nutmeg was introduced to Jamaica about 1782, and to St. Vincent somewhere about the year 1809; later it was taken to other West Indian islands. It is now an important source of revenue to some of our West Indian islands. Grenada, for instance, in 1921 exported nutmegs to the value of 60,712*l*.

The clove, like the nutmeg, is a native of the Spice Islands—the Moluccas—and now is the chief source of revenue to Pemba and Zanzibar. Cloves were probably introduced to Zanzibar, Fiji and Mauritius about 1797, and somewhat later to Trinidad.

Tea (*Camellia Theifera*, or as it is now called *Thea sinensis*), is a native of China, and is an important economic product of India and Ceylon, having replaced the coffee which was formerly grown in Ceylon until it was destroyed by the fungus *Hemileia*. It was first cultivated in India in 1834, having been brought by Gordon from China, but the more important introduction was by Fortune in 1848, when he brought plants from China in Wardian cases. Bohea tea was introduced to Jamaica as long ago as 1771 and 1797, and Assam tea was sent to Jamaica from Kew about 1848–1857, and again in 1868. The industry in Jamaica is unfortunately in a languishing condition, largely due to the high cost of labour. Tea was also introduced to Natal from Kew in 1850, and it is being grown successfully in Nyasaland, the first exports from Nyasaland being made in 1908. It was also introduced to the Cameroons by the Germans, and a small experimental plot may be seen at Buea, some 3,000 feet above sea-level.

Another economic plant which should be of value to our colonies is the camphor tree (*Cinnamomum Camphora*), but in this plant we are confronted with a peculiar economic problem. There are two forms of the camphor tree, one of which yields solid camphor on distillation and the other mainly camphor oil. There is no botanical difference between these two varietal forms, and it seems probable that it may be a climatic factor whether the plant will yield oil or solid camphor on distillation. Apparently in the cooler climates solid camphor is yielded on distillation, but in warmer places the trees generally yield oil. The problem, therefore, would appear to be, if camphor is to be a successful commercial enterprise in our tropical colonies, to find trees which will yield solid camphor when grown under tropical conditions. The coffee plant illustrates another difficulty which may attend the introduction of an economic plant to a country in which it is not native. It was introduced to Ceylon from

Africa about 1825, and was a flourishing industry in the island for many years. In Africa it has managed to establish a harmony with the parasitic fungus, and the coffee does not materially suffer. In Ceylon, however, the fungus appeared in 1868, and since then it has got the upper hand, and the coffee was so seriously affected that its cultivation had to be abandoned. Apparently the change of environment or some adverse climatic conditions rendered the coffee less resistant to the attack of the fungus, and so enabled it to get the upper hand and destroy its host.

Turning now to *Cinchona*, whose introduction has been a complete success, it was introduced to India from South America in 1853 ; but the first successful introduction took place in 1861, when the plants conveyed by Sir Clements Markham from Peru and Ecuador were brought to Kew and then taken to India. The value of *Cinchona* as a source of quinine is well known, and very large plantations exist in Java as well as in India. It was also introduced to Jamaica in 1861, where it flourished, but unfortunately its cultivation there has been neglected. The *Cinchona* plantations in India are not able to supply sufficient quinine to supply the needs of India and the Empire, and its cultivation is now being extended to Burma. The reports for the years 1924–25 indicate that the new plantations are proving very successful ; the plants are growing extremely well, and it seems probable the area will be extended from the Tenasserim river eastwards towards Siam. Should this be done it ought to be possible to produce sufficient quinine in India and Burma to affect controlled prices, and so bring the drug well within the reach of India's malarial millions.

It is interesting to note that *Cinchona* will grow in tropical Africa at about 3,000 feet, as it is now growing at Buea in the Cameroons, having been planted there by the Germans in about 1902. It is also flourishing at Amani in the Tanganyika Territory, and the trees there give a high yield of quinine.

Kew has been intimately concerned with the introduction of another economic plant, *Hevea brasiliensis*, the para rubber, which was brought from its native home in South America mainly through the instrumentality of Mr. Cross and Mr. (now Sir Henry) Wickham about 1875–78. There are various problems concerning *Hevea*, but the particular one to which I wish to refer is that *Hevea*, like camphor, shows different physiological varieties. Some plants are found to yield 2 per cent. and some as much as 40 per cent. of latex, which contains the rubber. The success of a plantation naturally depends on whether the rubber plants yield a high or low percentage of latex, and the problem now is to replace the poor-yielding plants by others giving a high yield. This may be effected either by budding or grafting or by raising young plants from trees known to give a good yield which have been fertilised with pollen from high-yielding trees. Unless the qualities of both parents are known the nature of the offspring is liable to be uncertain, and even then there is some element of risk, as the parents may be of mixed origin and may not represent "pure lines."

A brief reference may be made to one other economic plant, sisal hemp (*Agave sisalina*), from Mexico, which has been introduced to India, Ceylon and to our East and West African colonies. It was introduced to West Africa about 1890 and to East Africa in 1901, and is now proving a very valuable Empire source of fibre.

Certain aspects of the prehistoric and recorded introductions of economic plants have been referred to, and something must now be said about the modern aspects of the quest.

The problem before us at the present day is not so much the introduction of new economic plants, but the proper culture, amelioration and preservation from fungal and insect attack of the economic plants which are now being grown on a commercial scale.

The lines of inquiry relate to matters of soil chemistry and to the types of soil suitable for the different subjects under cultivation; to genetical questions which concern especially the sugar cane, cotton, the oil palm and the banana; to the selection of varieties immune to some particular disease, a matter of urgent importance in the case of the Panama disease of the banana, and in the case of the lime, which is being devastated, in Dominica especially, by the wither-tip fungus. There are also important questions awaiting solution in connection with the fertilisation of cacao, and physiological or perhaps soil questions relating to the die-back disease of the clove. These various problems can be easily stated, but their solution involves long and continuous research, and it is fortunate that we now possess in the Imperial College of Agriculture in Trinidad the appropriate place and the staff of scientific workers, who, in touch with other workers in the tropics, may be expected to throw much light on these problems and materially help forward their solution.

The production of better varieties, or of strains resistant to disease, is no easy matter, as will be seen from a study of a few concrete cases.

The modern quest with regard to the banana—for instance, in connection with Panama disease—bristles with many unexpected difficulties.

In the first place, the edible banana very rarely sets seed, and the problem here is to cross a seed-bearing variety or species which is immune to Panama disease with an edible form such as the Gros Michel, which is highly susceptible to the disease. From such a cross it should be possible to produce a plant which does not bear seed and is also resistant to Panama disease. Should such a result be obtained the problem is not necessarily solved, because it is essential that such a seedless banana should have a sufficiently thick skin to be a good traveller on board ship, so that it could be easily transported from the West Indies or elsewhere to England. The fruits must also be of a good size in order to find favour with the purchaser; and more important still is the arrangement of the fruits in the bunches. For packing purposes it is essential that the fruits be curved inwards towards the main stem, since if they curve outwards they take up too much space when packed, and are also very liable to damage when handled. It is, of course, an important point that the fruits should be of a good flavour, but except for the connoisseur, and the main banana consumers do not include many in their ranks, this is a somewhat secondary consideration. It is the size of the bananas and the arrangement of the fruits in the hands which are the really essential matters. In this connection it is interesting to refer to the giant fig variety which is being grown in Grenada. This banana has an excellent flavour, but it is not, under present conditions, wholly suitable for transport; a considerable amount of research is required as to the right temperature for storage on board ship, and the right time for picking, before this variety can be put on our markets successfully.

With regard to the breeding of immune strains of bananas by crossing the susceptible Gros Michel with the varieties imported from other countries, due consideration must of course be given to the great danger of importing new diseases, such as bunchy top, which is causing so much damage in the East.

The modern quest with regard to limes opens out somewhat similar problems. Limes at present are the staple industry of Dominica, and the present serious situation caused by the wither-tip disease forms a sad example of the danger of relying on a single economic product.

With regard to limes, the problem is to find a variety resistant to the wither-tip disease, caused by the fungus *Glœosporium limetticolum*, which was first recorded in Dominica in the year 1922. It is almost impossible to spray the plants in Dominica owing to the heavy rainfall and the steep hillsides on which the limes grow. The problem is, therefore, to find either a naturally resistant or immune variety in the West Indies which could be propagated from seed, or to make a survey of the cultivated limes of the world and see if an alternative variety or species can be discovered. There is also a possibility that a resistant variety may be found by artificial hybridisation, but owing to polyembryony in limes considerable research would be needed before useful results could be obtained.

As with the banana the production of a new form has a commercial aspect which raises considerable difficulties for the plant-breeder. It is essential that any new variety of lime that may be discovered or produced must be (1) a heavy cropper and the fruits should have a high citric acid content with the characteristic lime flavour, and (2) it is all important that the fruits should drop from the tree when they are ripe. It may of course be quite possible to produce a lime resistant to the disease which also yields a plentiful supply of fruit, but if these fruits require to be picked from the tree the whole value of the work of the plant-breeder would be useless on general economic grounds.

The modern problems with regard to the sugar cane relate partly to pathological matters and partly concern genetics. In the West Indies and elsewhere the sugar cane is attacked by a "virus" disease known as mosaic disease, which also attacks maize. Much damage results to the cane, and the exact nature and cure for this disease is a matter demanding long and careful research. Another serious disease of the sugar cane, which fortunately has not yet appeared in our colonies, is the sereh disease, which nearly destroyed sugar cultivation in Java.

By crossing the cultivated cane with the wild grass-like *Saccharum spontaneum*, which is immune to the disease, and also by cross fertilising the sugar cane, *S. officinale*, with chuni and other small Indian canes, immune hybrids with a good sugar yield have been produced, and are now in general cultivation in Java and other Dutch islands, as well as in British colonies. The Dutch, realising the great importance of taking the fullest measures for dealing with this disease, established a special research station for the work at Passaroeen in Java, which the results have amply justified.

Genetical research is also required in order to try and produce *stable* varieties of sugar cane, instead of having to depend on the selection of chance seedlings whose characters are not fixed.

The sugar industry owes a lasting debt to Mr. J. R. Bovell and to the late Sir John Harrison for their work on seedling sugar canes, and their discovery that the sugar cane occasionally sets seed. Now, however, the time is ripe for the laborious and difficult task of carrying out breeding work on Mendelian lines with the sugar cane, on lines similar to and with, it is to be hoped, results analogous to those which have resulted from the Mendelian researches on wheat.

Cotton offers somewhat similar problems, and much is to be expected from the prosecution of genetical experiments with this important economic crop.

With the oil palm there are at least two important problems: one connected with genetical investigations, the other being cultural. Throughout our West African colonies the oil palm abounds, but there are many distinct varieties, differing both in the amount and chemical composition of the pericarp and kernel oils, and also differing very greatly in the thickness of the shell. These varieties are to be found growing intermixed, especially in the Gold Coast and in Nigeria.

To ensure ease in handling by machinery and a uniform type of oil, a palm is required which will yield nuts of a certain and constant thickness of shell, and oil of a definite and fixed chemical composition.

A palm with such a combination of desirable characters should not be beyond the scope of careful genetic research. It would, however, be of little value were such a palm produced in West Africa to-day under the conditions which at present obtain, as the ground is already covered with individually-owned palms of every sort and character; and if West Africa is to compete successfully with the well-arranged oil palm plantations in the Dutch East Indies, it is essential not only that the best type of palm should be grown, but also that it should be grown under proper plantation conditions.

Two other economic crops may be referred to in conclusion—first, cloves and the die-back disease; and, secondly, cacao.

The clove disease is due to a fungus, but the presence of the fungus has been found to be the *symptom* rather than the *cause* of the trouble. As is so often the case with disease, the cloves in certain areas in Pemba were found to be dying-back because they were growing in unfavourable soil conditions. These plants were therefore in a weaker condition, and unable to withstand the ravages of the fungus. A case of this sort, which is somewhat parallel to that of the lime, is one demanding research on physiological lines as well as researches into the geological conditions and questions of soil chemistry.

Cacao (*Theobroma Cacao*) affords several interesting fields for study, all of which require careful research. In the first place we know very little about the fertilisation of cacao, or by what insects the flowers are fertilised. Then it appears possible, from the recent work of Prof. Pittier in Venezuela, that all our cultivated cacaos may be hybrids between two wild South American species, *T. cacao* L. (*Forastero*) and *T. leiocarpum* Bernoulli (*Cabacacillo*). If this be so it may explain why some cultivated cacaos yield so little fruit, since the pollen may be bad or the ovaries infertile, and why, since plants are mainly raised from seed, all sorts of intermediate types have resulted. Further careful work is needed on the cacao flower, and horticultural work on the budding and grafting of good-yielding strains, which has been so successfully carried out in Dominica and Trinidad, should also be extended. Cacao, like other economic plants, has its attendant diseases which require all the skill of mycologists to keep in control, and the witches' broom disease in Ecuador, caused by a *Monilia*, is a pest to be watched for, as it has not yet invaded our cacao-growing colonies.

It is fortunate that for the study of all the pressing problems relating to economic plants we have, as I have said, in the Imperial College of Agriculture at Trinidad, not only an ideal centre for research in tropical agricultural problems, but also a staff of scientific experts competent to carry out researches in many of the lines to which I have referred.

It is to be hoped also that the Research Station at Amani will also shortly be available for research work of a similar nature with regard to problems affecting the economic plants cultivated in East Africa.

In these latter days, as I have attempted to point out, the quest for economic plants has assumed a new aspect. Partly owing to the extensive cultivation of economic products under plantation conditions, and partly to the great shipping facilities and the liability to the carrying of disease, our quest is now very largely one of finding varieties that will not only give a high yield, but will also be resistant to the diseases which everywhere are manifest, and which can so easily be spread under plantation conditions. The cultivation of economic crops under the best possible conditions, the identification and suppression of diseases and the production of new and improved races, and many other problems provide serious occupation for the plant physiologist, the pathologist, the agricultural chemist and the geneticist, and tend to keep alive the romance of economic botany.

It may well be said in the words of the writer of the book Ecclesiasticus: "When a man thinketh he has finished, then he is but at the beginning, and when he ceaseth then shall he be in perplexity."

[A. W. H.]

WEEKLY EVENING MEETING,

Friday, April 23, 1926.

Sir Edward Pollock, F.R.C.S., Manager and Vice-President,
in the Chair.

Richard Whiddington, M.A. D.Sc. F.R.S., Cavendish Professor
of Physics, University of Leeds.

The Luminous Discharge through Rare Gases.

[Summary.]

When a current from a storage battery is passed through a vacuum tube containing Argon or Neon at low pressures, the discharge presents the usual ordinary appearance of positive column to the eye.

When viewed in a rotating mirror, the luminosity is seen to be regularly intermittent, consisting of brilliant coloured flashes moving quickly down the tube from anode to cathode.

The speed of these flashes is inversely proportional to the pressure of the gas, is determined to some extent by the value of the current, but is not much affected by the applied potential.

When the gas is impure the flashes are non-uniform, sometimes evidenced by a general wavy appearance in the mirror, sometimes by two clearly differentiated sets of flashes of differing speeds in two parts of the tube.

Possible theories of this new type of discharge are mentioned, and its relation to the well-known discharge phenomena discussed.

The form and character of the luminous discharge in the ordinary gases under conditions of low pressure and constant potential are quite well known. It will, therefore, be sufficient to pass these facts rapidly in review as an introductory to the new phenomena in the case of the rare gases.

A long glass tube evacuated to a pressure of 1 mm. of mercury or thereabouts, and furnished with plane electrodes, will, when fed with current from a constant potential of a hundred or so volts, present a characteristic appearance: the most important visible parts, proceeding along the tube away from the cathode, being the cathode dark space in which the luminosity is relatively faint, the negative glow, the Faraday dark space, and then, filling the majority of the tube if it be a long one, the positive column.

The cathode phenomena in this ordinary type of discharge are perhaps the most arresting, and it may not be out of place for our present purpose to notice in passing the explanations and to perform one or two experiments.

The cathode is, of course, the seat of the cathode rays which stream away down the tube, ionising in their course. They are produced at or near the surface of the cathode by the impact of positive ions which originate most plentifully at and beyond the luminous boundary of the dark space. This space is dark because of the very high electric force obtaining there due to the space charge action of these positive ions, which high force prevents the re-combination essential to luminous emission. The dark space edge, therefore, marks the point at which the electric force has fallen to so low a value that the positive ions and the electrons from which they have been forced to part company are not so very rapidly swept past each other. It is a region of high conductivity. That this is the region from which the positive ions bombarding the cathode to produce electrons are drawn may be shown by this experiment.

A specially designed tube is furnished with a movable anode, which, when moved towards the cathode, produces no effect beyond the gradual disappearance of the part of the discharge through which it advances. The milliameter shows that the strength of the current is approximately maintained until the anode reaches the edge of the dark space. The current then suddenly stops. If the anode be moved back now, ever so slightly, the current begins to flow once more and luminosity reappears.

In passing, too, an interesting matter may be noted. If the anode is screwed back just to the point at which the discharge may be expected to recommence, but no further, a considerable delay may take place before it appears. If, however, a sample of radium be brought up, the current at once commences to flow, and we see the very thin layer of negative glow proper to the conditions of the tube.

The explanation in all probability is that a stray ion or so is necessary to the initiation of the discharge, and we are perhaps dependent on the natural ionization produced, maybe by the hard, all-pervading cosmic rays.

With the tube in the condition we chose, there is only a very thin layer of gas containing a comparatively small number of molecules available to this ionizing agency. The chance of ionisation is small; we must wait for it. If impatient we can increase the chance enormously by bringing up the stronger ionizing agent. The same result, of course, could have been effected by withdrawing the anode a little further.

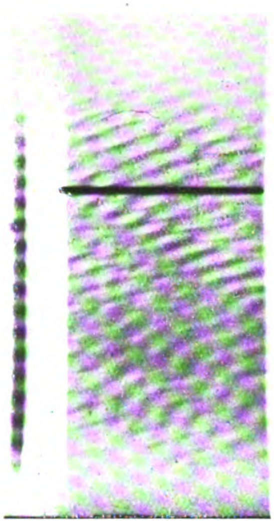

Fig. 1.

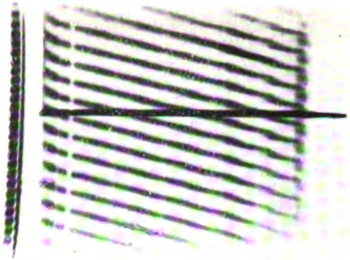

Fig. 2.

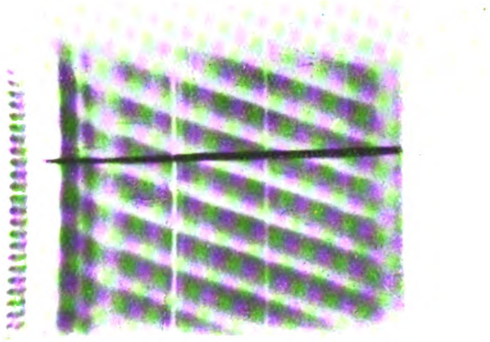

Fig. 3.

The important point, however, is that at a given gas pressure the anode and cathode must be more than a certain distance apart or the discharge goes out.

Reverting now to the tube in which the electrodes are a considerable distance apart, it should be pointed out that whereas very close to the cathode the field is determined in large measure by the potential applied, yet as points further and further away are considered the space charges become more and more important, until, when the positive column itself is reached, these space charges take control of the field almost entirely. Closely connected too with this matter of the space charge is the wall charge. This arises because both electrons and positive ions diffuse, the former more readily than the latter, with the result that the walls become negatively charged. There is thus set up a radial component of current which is of some importance in quantitative considerations.

The presence of this wall charge is rather well shown by an experiment in which the cathode is surrounded by a movable glass sheath. When this sheath is moved out over the cathode the pencil of cathode rays, originally broad, is compressed to a far narrower pencil as a result of the action of this negative wall charge.

It is a matter of some interest to notice here that experimenters with the luminous discharge have often found difficulty in maintaining the current perfectly steady. There is always a tendency for intermittency to appear, a regular oscillation often of very high frequency causing a note or hiss, for example, to be apparent in a telephone receiver included in the circuit. Yet in the striated form of discharge at any rate, viewing the tube in a rotating mirror reveals usually no discontinuities.

When a discharge tube containing a rare gas—argon for example—comes to be examined, new features are at once apparent. When the continuous positive column is viewed in a rotating mirror very obvious variations in luminosity appear.

When no special pains are taken to ensure purity of the gas, the appearance is very irregular (Fig. 1), but with argon carefully purified from hydrogen and nitrogen by the usual means, a very beautiful and regular appearance is presented (Fig. 2).

This can only be due to a perfectly regular sequence of luminous flashes travelling with approximately uniform speed down the tube from anode to cathode. Viewed directly, the so-called uniform positive column only is seen; viewed in the mirror its lack of uniformity is immediately obvious.

The inclination of the flashes is a measure of their speed, and by taking instantaneous photographs of the tube in the mirror simultaneously with a small mercury vapour arc fed with alternating current of power high frequency to act as a time-marker, the actual velocity of the flashes can be calculated. The speed varies with the pressure from about 10^3 to 10^6 cms. per second, and hardly depends at all on the potential applied to the tube except in so far as this changes the current.

Very approximately the product of the speed and the pressure is constant, at any rate over ranges not too large.

The way in which the speed falls off with pressure is strikingly illustrated in Figs. 2, 3 and 4, the steeper (slower) flashes corresponding to the greater pressures.

It will be noticed, too, that the spacing between the flashes along the tube is roughly independent of the pressure over the range here considered. In other words, the tube contains on the average as many flashes at any instant at all pressures. Moreover, so far as the eye can judge, the flashes appear to be equally spaced.

When the tube has been running for a short time hydrogen appears in the tube, and in addition to the steep flashes a new, narrower and more horizontal set appears nearer the cathode, as in Fig. 5.

The spectrum reveals argon lines all along the tube, with the addition of hydrogen lines—strong near the anode—and are weak or absent near the anode.

It is natural to suggest that these flashes (or moving striations) consist of positive ions moving away from the anode; but it is by no means certain that this is the true explanation, and it will be used merely in a tentative manner, while an alternative, and possibly more likely, hypothesis will be introduced at a later stage.

An important point to notice is that so far as experiments with argon up to the present go, the spectrum of the discharge shows no vestige of the Döppler effect when viewed along the tube,* whereas the resolving power of the spectroscope was certainly high enough to show the effect had it been present; it is to be supposed then that the motion of the flashes is apparent only, and may be regarded as a state of affairs consequent on the passage of "non-luminous" positive ions through the argon.

Although it has not been found possible to find a quantitative theory of this type of discharge owing to its very complicated and varying forms, it may be worth while here to suggest the manner in which these flashes may be generated and propagated on the view just mentioned.

The space near the anode will contain electrons which at the moment of applying the potential will commence to move up to the anode. Only those electrons originating from a plane further than a certain distance away from the anode will acquire ionising speed at the anode surface. When this sheet of positive ions is formed it

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* The experiments at the time were thought to be fairly conclusive, but it has since been realised that flashes thought to have been due to argon may have been really hydrogen, which furnishes spectral lines unsuitable for observation at the temperature of working of the tube. The argument now put forward must therefore be regarded as tentative.

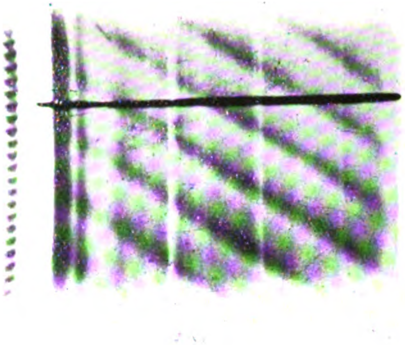

FIG. 4.

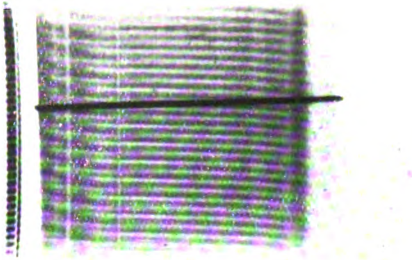

FIG. 5.

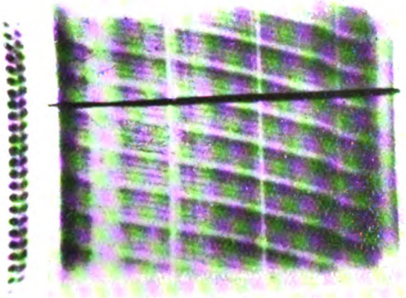

FIG. 6.

commences to move away from the anode as a space charge, which neutralises the field just behind it. The sheet is, therefore, quite thin, and moves away down the tube, its motion being marked by a patch of light apparently moving with it. The sheet may be regarded as acting as a virtual anode, attracting electrons to itself and so keeping up the supply of positive ions.

When it has moved a sufficient distance away a fresh sheet of positive ions is generated in the same way, which again moves off in its turn from the anode ; thus a regular sequence of flashes is set up. At some distance away from the anode the motion of a flash is to be regarded as kept up by the repulsion of its successor.

If this be so it might be expected that the potential applied to the electrodes should not affect very much the speed of the flashes. That this is roughly true may be demonstrated by using a tube (containing neon in this case) of the kind used so extensively to-day for advertisement purposes. If an alternating potential of about 800 volts and 50 ~ be applied, the characteristic and apparently uniform positive column is seen. On rotating a mirror slowly the field in the mirror is seen to be cut up by parallel dark lines corresponding to the zero points in the alternating potential. On rotating the mirror more quickly the bright field is seen to be crossed by rather wavy lines clearly of the type we have seen in argon, which reverse their slope at every half cycle. By suitably choosing the speed of our mirror we can select one half of the cycle only, so that attention is concentrated on flashes moving in only one direction down the tube. It is clear when this experiment is performed that, apart from irregularities due to impurities in the neon, the inclination of the lines seen in the mirror are about the same : they are all parallel and straight—that is, their speed in the body of the tube are all the same and independent of the applied potential. Near the electrodes, of course, this is not the case.

The speed of the flashes for any one gas appears to depend mainly on two factors—the current and the pressure. Over a limited range of gas pressure it has been found that the product of the pressure and the velocity is approximately constant. This means, of course, as our view of the phenomena would suggest, that at greater gas pressures the flashes move more slowly.

An interesting point is that over a very wide range of pressure, and therefore velocity, the flash spacing remains approximately the same ; there are at any instant always about the same number of flashes present in the tube (usually three or four only with the tubes ordinarily used).

If the same charge is associated with every flash, this is what would be expected on our present view if below ionising speed the electrons suffer perfectly elastic impacts. The explanation—qualitative as it is—cannot really be so simple as this, for reasons which will be obvious from what follows.

Multiple excitation easily occurs in the rare gases, and evidence of this effect is not wanting in these discharges. The flashes often double, still retaining the same velocity, while an extreme case is shown in Fig. 6, where four distinct sets are shown in place of one.

If it were certain that the current in the tube was carried entirely by the flashes, it would be possible to calculate the charge associated with each flash, and thus the potential in the body of the tube under which the exciting and ionising electrons move. Attempts in this direction give the result that the potential fall involved is about 6 volts. The blue spectrum of argon is present, for which the lowest voltage of excitation so far recorded for high current densities is 19 volts. As the currents used in the present experiments run up to more than an ampere, it is possible that multiple excitation may account for this very low value. The calculation, however, rests on very doubtful assumptions, and no emphasis can be laid upon it. A very serious difficulty is that while the majority of the luminosity from the tube originates in the flashes, there is a distinct luminous background—whether emitting the same spectrum or not cannot so far be said; this is almost certainly associated with conductivity, and goes to vitiate any argument of the kind suggested.

When the flashes are followed up to the hot cathode, it is observed that their speed rapidly drops. This is doubtless due to the high conductivity of the gas in this region acting to reduce the electric force there.

The question of these moving flashes is by no means cleared up yet, for there are two very serious experimental objections to the hypothesis just outlined.

The first is, that if there is a considerable convection of gas in the form of ions along the tube, it should certainly be possible to detect a pressure distribution. This has not so far been observed.

The second is, that in the majority of cases so far examined oscillograph determinations of the current show no intermittency, or very little.

These objections would be removed, and most of the phenomena described would be explicable, if it be supposed that the flashes are due to atomic excitation and not ionisation.

The explanation would be somewhat as follows:—Electrons initiated in the cathode region and accelerated along the tube would attain energy sufficient for excitation at some definite plane, producing there a sheet of luminosity.

Just on the anode side of this plane there would thus be brought into existence a sheet of electrons at rest, constituting a space charge. Electrons would move away from this region and accelerate until they in there turn produced further along the tube a second sheet of luminosity, and so the process would be repeated.

On this view there is so far no reason to see why the luminosity should move towards the cathode. There must clearly be some process at work which prepares in advance the atoms on the cathode side of the flash for impending excitation. This may be accomplished by actual diffusion of the excited atoms themselves, by radiation from them, or in some manner combining the two; but since no definite conclusion is possible at present, this bare mention of the speculation may suffice.

We come, in conclusion, to a rather interesting consideration which arises from the possibility of more than one set of flashes of different speeds existing at the same time in the tube. This might occur in the case of a mixture of gases.

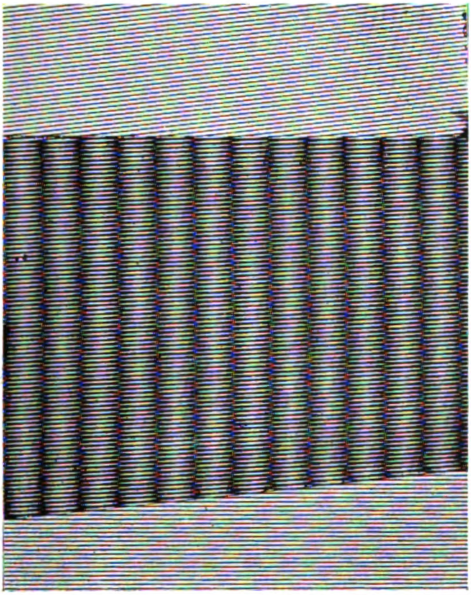

FIG. 7.

An impression of what the combined effect of two sets of flashes of unequal speed would be in the rotating mirror is given in Fig. 7, which is the result of superposing two coarse-ruled transparent gratings. This "moiré" effect gives straight, parallel bands, whose direction is that of the shorter diagonal of the parallelograms formed by the lines of the gratings. The inclination of these combination bands may be varied by turning one of the gratings.

It may be that this effect can be made a basis for the explanation of the stationary striations seen in mixtures of gases carrying currents at low pressures.

[R. W.]

WEEKLY EVENING MEETING,

Friday, April 30, 1926.

SIR EDWARD POLLOCK, F.R.C.S., Manager and Vice-President, in the Chair.

W. H. ECCLES, D.Sc. F.R.S. M.R.I.,
Vice-Chairman, Wireless Telegraphy Commission.

Wireless in the Empire.

THE British Empire is a medley of fragments spread as if by blind chance over the habitable portions of the globe. Looked at in Mercator's projection it is like a broken potsherd on the floor—a collection of large pieces and small pieces, at all distances apart. The small pieces are so numerous that the man in the moon, who has watched the growth of the Empire through the centuries, must think that Britain is the Autolycus of the nations—a snapper-up of unconsidered trifles—as well as a lion with the lion's share of the wide spaces. Yet, scattered as they are, these pieces of earth bear populations which instinctively turn to the mother country as the centre of that world which lies beyond their immediate interests. This is as true of the large pieces as of the small. I happened to be in Australia in July and August of 1914, and saw the great crowds in the streets of Sydney waiting all night, and night after night, for the cablegrams that dribbled through to tell us of the outbreak of war and of the first movements of our Fleet against the enemy. Doubtless similar anxiety for the welfare of the mother country united the whole Empire, scattered though it is.

For so dispersed a family the problem of telegraphic communication is important and difficult. Until the year 1902 long-distance telegrams were all carried by cables, under the sea or under the ground, and by wires on poles; and the task of serving sparsely populated and distant regions required capital outlay beyond reason. But between 1902 and 1904, in a series of bold experiments, Marconi proved that messages could be conveyed thousands of miles by telegraphy without wires. Then he and many others showed that electric waves could pass over mountain chains, and even far round the bend of the globe, at any rate at night, and that they could be picked up simultaneously in all directions round their source. Here obviously was a heaven-sent gift to the British Empire, for by wireless telegraphy we might now hope to communicate, simultaneously if desired, with a thousand places to which cables and wires could never

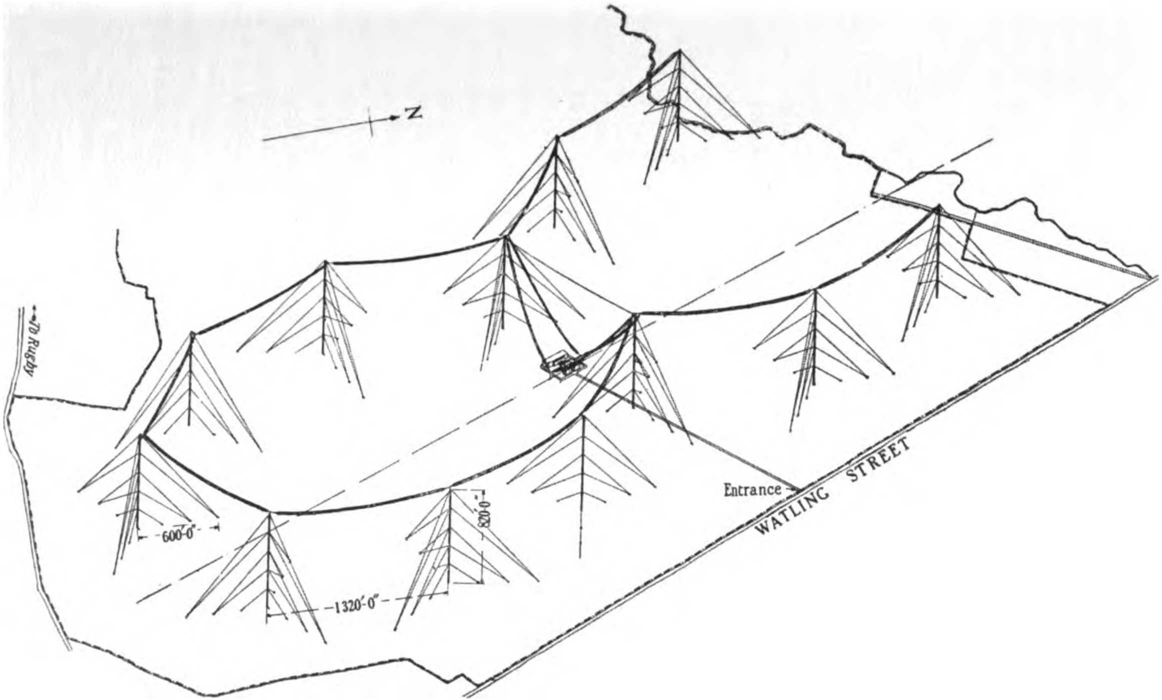

FIG. 1.—DIAGRAMMATIC VIEW OF MASTS AND ANTENNA AT RUGBY STATION.

be run. The progress made in the utilisation of this gift of science is the subject of my Discourse this evening.

Quite early in the development of wireless telegraphy the principal nations began to apply it and to regulate its employment. Indeed control was compelled, in the armed camp that Europe then was, by the consideration that, when war came, wireless would be useful to fleets and armies and dangerous in the hands of spies. In peace time, even, some degree of control was called for to reduce interference, especially between powerful long-range stations. Each such station ought, in fact, to transmit on a wavelength different from other stations; and once a wavelength is assigned to a particular long-range station, it should not be given to any other. In other words, the grant of a wavelength to a powerful station is equivalent to giving a right-of-way to that station. For all these reasons the Imperial Conference of 1911 declared that wireless communication within the Empire should always be regulated with especial care, and that, in the main, the necessary stations should be State-owned and State-operated.

This decision being given it only remained to erect the stations. In 1912 Parliament approved a contract which the Post Office had made with the Marconi Company for the erection of an Imperial chain of six stations. This contract was, however, modified after a technical enquiry, and a new contract was ratified by the House of Commons in 1913. But the engineers had barely time to erect a few masts at Leafield in Oxfordshire and at Abu Zabal near Cairo, when the contract was dissolved, principally because of the advent of war. In contrast with our inaction, the United States had meanwhile pressed on with the construction of several large government stations, and France and Germany made great progress with their respective colonial plans. Perhaps we could afford to delay. Compared with Germany, for instance, we possessed elaborate cable communications, and were more confident about defending them in every sea than Germany could be. But the result was that when war broke out in August 1914 no unit of the much-discussed British wireless chain had been erected.

Germany, on the contrary, had made great strides with her colonial communications between the years 1912 and 1914. She had established thoroughly modern stations in Togoland, in German East Africa, in German South-West Africa, and in the Pacific. The three African stations could inter-communicate with one another, and, to a limited extent, with the great station at Nauen, near Berlin. The Pacific station was at Yap in the Caroline Islands. It was a noteworthy coincidence that all these stations were completed just before the war began. I remember hearing the early trial signals from Yap while I was sailing across the Pacific in July 1914. No doubt this station was intended to receive instructions from Berlin, and re-transmit them to island possessions, such as Apia in Samoa, and to naval and mercantile vessels in the Pacific and the China Seas. It may be of interest to recall the fate of Yap; it is wrapped up with the story of the German Pacific fleet commanded by Von Spee which, months later, met Cradock at Coronel and Sturdee at the Falkland Islands. I remember some of the story because I was called from Sydney to Melbourne by the Australian Government to help in an advisory way with an interesting application of wireless. At this moment the German fleet was cruising to the north of Australia and was in nightly touch with Yap. As soon as this was observed each of the small shore stations along the Australian coast was instructed to listen for the signals of the German fleet, to record the strength with which the signals were received, and to report daily to Melbourne. From these reports, by a process resembling triangulation, an estimate was made in Melbourne of the changes of position of the fleet. Meanwhile a British naval force had set forth with the object of destroying the wireless station at Yap. This was duly accomplished, and for days afterwards the Germans, unaware of the disaster, continued to call the dead station, in vain. These continued calls, by helping us to keep track of the fleet, eased the anxiety of those who knew that the New Zealand expedition was then well on its way to capture Samoa. Thus ends the story of Yap; that of the other German colonial stations was similar.

During the war our wireless energies were deflected from the building of great stations to producing relatively small outfits for fighting purposes. War telegrams were handled sufficiently well by the cables and the wireless stations of the Admiralty. And the war closed without any progress in imperial wireless. Meanwhile the French Government had erected powerful stations at Nantes, Lyons and Paris, and smaller stations in their African territories; the Italian Government had erected a large modern station at Rome; and the United States Government had completed half-a-dozen big stations, together with a magnificent one near Bordeaux which was afterwards purchased by the French Government. But the governments of the British Empire, at the close of the war, stood possessed of only one fairly large wireless station among them—that at Windhuk in German South-West Africa—captured by the forces of the Union of South Africa under General Smuts. Thus at the end of 1918 the Empire was far behind most other great Powers as regards government-owned stations, and, by the way, equally far behind as regards powerful commercial stations.

At this instant Lord Milner took up the cause of imperial communications. Lord Milner appears to have been the only elder statesman of that day convinced of the imperial value of widespread communications, and aware of our national backwardness. But he looked for much more than mere duplication of cable and land-line services. He envisaged the possibility of radiating daily

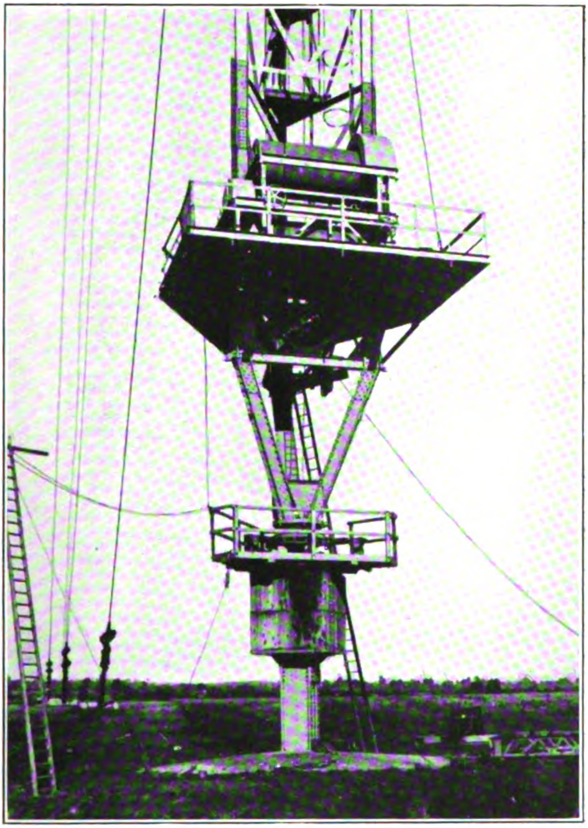

FIG. 2.—BASE OF ONE OF THE 820 FEET MASTS.

G 2

from England and other great centres news and opinions of imperial interest in such a form that they could be picked up, perhaps after re-transmission by outpost stations, anywhere. He thought it wrong to continue to leave a large proportion of our Empire population as isolated as if cast away on a raft in the ocean. I believe he thought that wireless would be as useful for the cultivation of mutual understanding as for strategy and commerce. In other words, he realised fully the importance of propaganda, if you care to use the word in imperial politics, and holding these views advocated government expenditure on improving Empire communications.

Within a year of the conclusion of the war Lord Milner appointed a committee, under the chairmanship of Sir Henry Norman, to investigate afresh the problem of establishing a comprehensive wireless network. The committee found that the progress made in wireless during the war had completely changed the outlook. Plant was now available of at least double the effectiveness of the pre-war wireless chain proposals. Accordingly the Norman committee recommended that wireless stations of about 250 kilowatts power, giving 120 kilowatts of continuous wave power to the antenna, should be erected in Egypt, East Africa, South Africa, India, Singapore, Hong Kong and Australia, with, possibly, similar stations in Canada for transatlantic and transpacific communication; and that a station of double the power of the others should be erected in England. Stations such as these, it was thought, would each be able to communicate at all hours with its nearest colleague even under the very bad conditions that afflict wireless reception in the tropics; and each would communicate with its most distant colleague during at least a few hours daily. The scheme, it will be seen, ensured that each point would communicate with every other at all times; directly when conditions were good, indirectly—that is, by the relay or passing-on principle as used in cable telegraphy—when conditions were bad. The English station being the most powerful of the network would have the maximum amount of direct communication with all. Besides this, the scheme would enable the Admiralty to transmit instructions from Whitehall to ships afloat anywhere, a requirement included in the terms of reference of the committee.

At this date there were no wireless stations in existence as powerful as those recommended, and, therefore, it was difficult to forecast what their performance would be. Moreover, the committee recommended that the stations should be equipped with thermionic valves. Now the largest valve station then known was of less than 20 kilowatts output, so the recommended English station was to be more than twelve times as powerful as its largest predecessor. The Report was, clearly, somewhat ahead of its time, but not, I think, too far ahead. For by the year 1925, only five years later, the Post Office station at Rugby has been built with thermionic valves yielding double the output recommended by the committee. The

Rugby station has shown, besides, during the tests of the past few months, what the Norman committee's stations could have done. For example, the station recommended for South Africa would have communicated direct with England, Australia and India many hours each day, and the English station would have worked direct to the antipodes more than twelve hours daily.

Lord Milner on receiving the Report was, I believe, especially pleased that stations were proposed for some of the dependent Colonies, such as East Africa, Singapore and Hong Kong; not merely because they would serve at times as telegraphic links between England and some of the great Dominions, but as ends in themselves. These intermediate stations would bring great areas of new country into closer touch with all other parts of the Empire. As you know, Lord Milner was of opinion that the dependent Colonies had for many years past been neglected, and that wireless telegraphy would help to alter this state of things. So at the end of 1920 the Cabinet appointed the Wireless Telegraphy Commission, of which Lord Milner became chairman, and officially assigned to it the task of getting as much done as it possibly could.

As no station could be erected until the requirements of all parties could be co-ordinated, the first task was to obtain unanimous consent to the scheme or a modification of it. It was unfortunate that Lord Milner retired from political life before any decisions were reached by the Dominion Governments. As it turned out each Dominion Government held a different view about Imperial wireless, partly as to the financing and management of it, partly about the technical requirements and possibilities. The only common element was that they each desired to communicate with England without intermediate steps. Thus as fast as each Dominion settled its policy a new compromise had to be framed for the Cabinet's approval. And very difficult it was to get Imperial wireless on to the agenda paper and have it discussed when, for instance, fabric gloves were agitating politicians. But by the powerful aid of the permanent officials concerned, the interest of Ministers was maintained during several months of waiting Finally, when Mr. Amery was chairman of the Imperial Communications Committee in 1923, Empire wireless took a new spring. The Government decided to erect in England without further delay a wireless station powerful enough to transmit almost continuously to all the Dominions and dependent Colonies, and to all ships of the navy and the mercantile marine wherever they might be.

The time of waiting had not been lost. The Commission had proceeded a long way with the plans of a large station, and the Post Office engineers had gained much experience by equipping with up-to-date plant the medium power stations at Leafield and Abu Zabal, remnants of the old Imperial scheme of 1913. As a consequence the erection of the Rugby station, the most powerful in

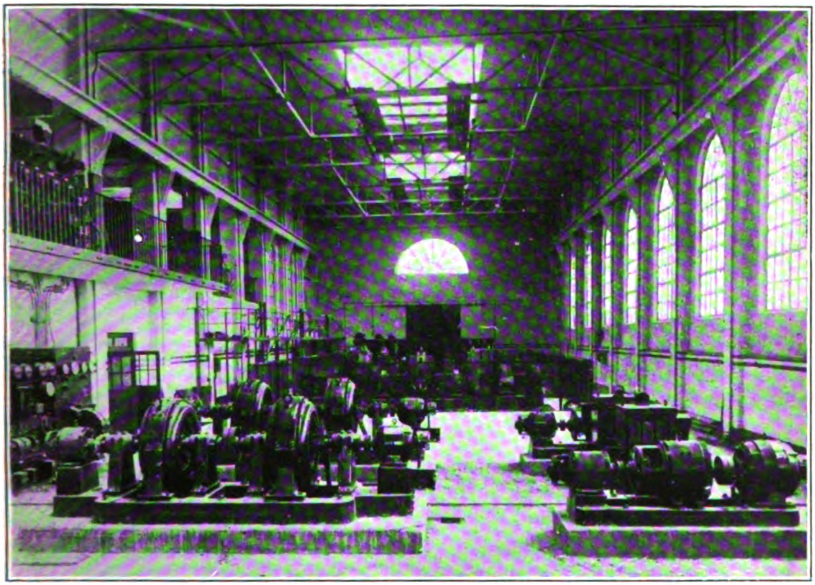

FIG. 3.—MACHINERY HALL, RUGBY.

the world, has gone forward with exemplary smoothness and without accident or failure of any kind. The station is full of novel features. (Figs. 1 to 6 give a selection of the lantern slides.) The station also contains the apparatus for the transatlantic telephony experiment. The ultimate aim of this experiment is to connect any telephone subscriber in this country to any subscriber in the United States and Canada. By aid of the telephone circuits already constructed between England and the Continent it may be found possible later to connect anyone in Western Europe to anyone in North America through Rugby. From this brief description you will understand that the Rugby station when in full swing will probably be able to

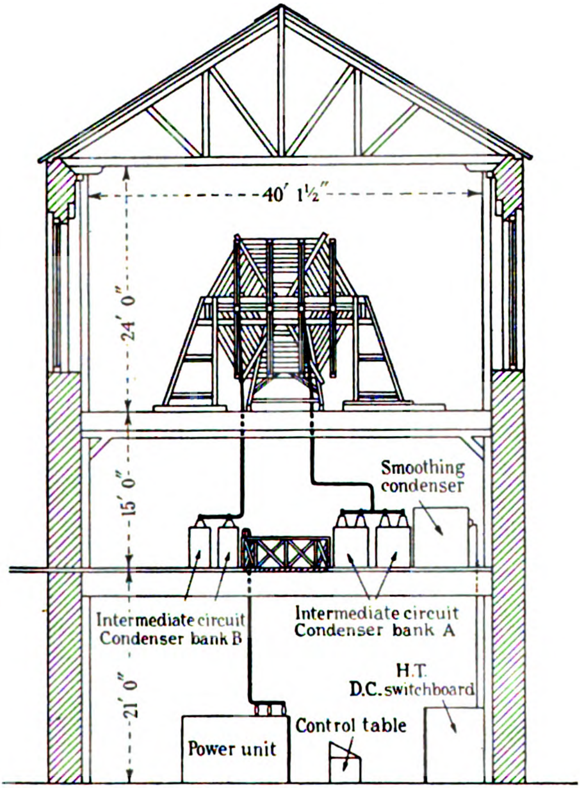

FIG. 4.—DIAGRAMMATIC SECTION OF HIGH-FREQUENCY BUILDING.

transmit simultaneously three long-distance telegrams and a telephone message.

When one realises that this multiple service station has cost only about half what the single service station near Bordeaux cost in

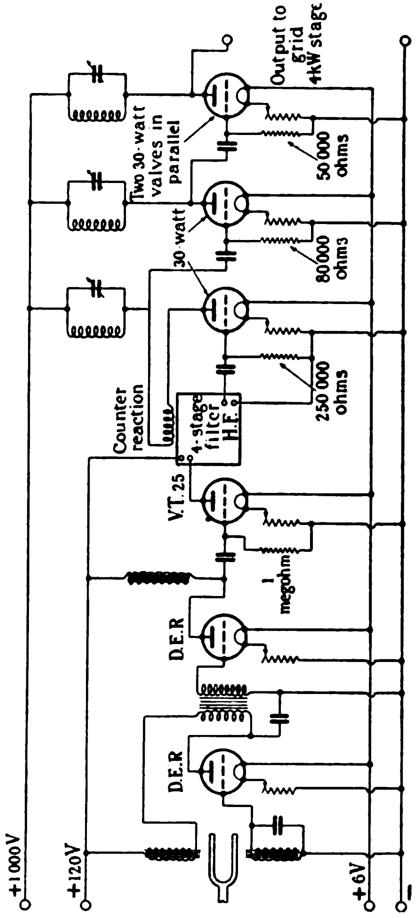

FIG. 5.—TUNING-FORK CIRCUIT FOR GENERATING THE WAVES RADIATED FROM RUGBY.

1919, one appreciates the great strides that wireless design has made during the past few years. Rugby, in fact, has cost about £400,000, much less than even the new stations at St. Assise near Paris and on Long Island near New York. Further, as the Leafield station began to earn, within a year of its opening, more than covered its working

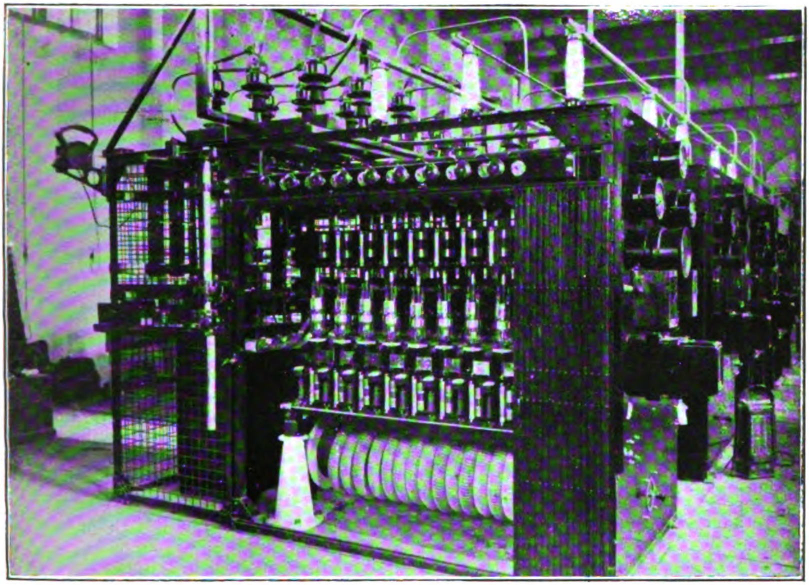

FIG. 6.—VIEW OF CAGES CONTAINING THE THERMIONIC VALVES. The gate of the first cage open.

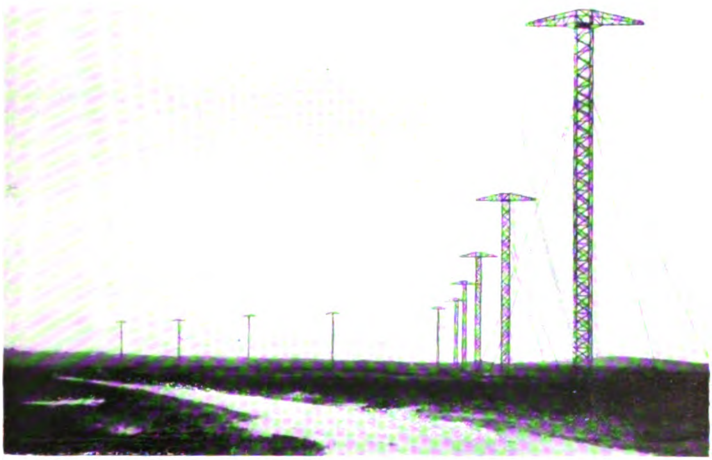

FIG. 7.—MASTS AT BODMIN BEAM STATION.

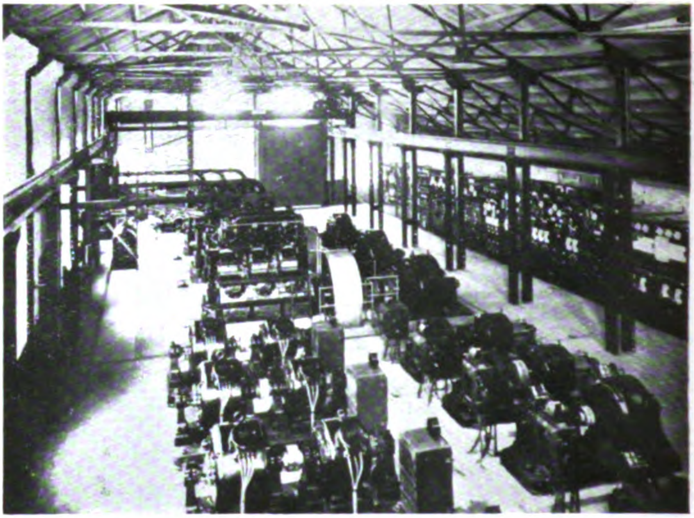

FIG. 8.—MACHINERY HALL, BODMIN.

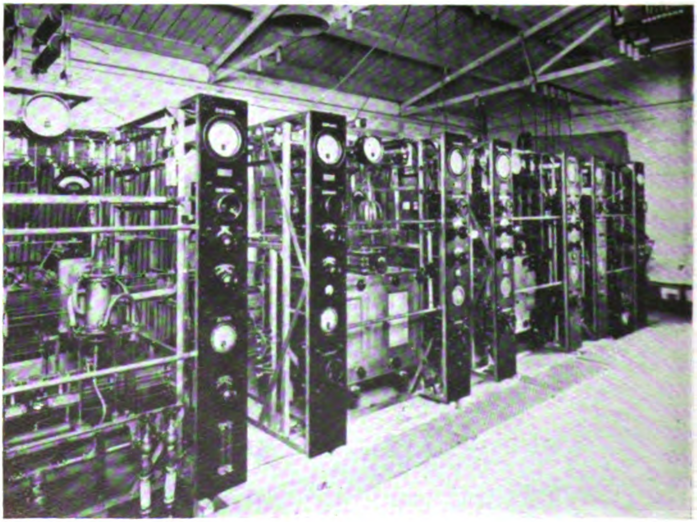

FIG. 9.—BODMIN TRANSMITTER.

expenses, so we may reasonably look forward to the Rugby station soon becoming a profit-earning concern.

It has always been obvious that the telegraphic traffic to and from the centre of the Empire will be greater than that to and from any of the Dominions: for the centre has to deal with all the Dominions, all the Dependencies and with distant shipping. The spokes all meet at the hub of the wheel. Therefore Britain must make much greater provision than even the largest Dominion, perhaps tenfold. Definite recommendations for the erection in England of stations additional to Rugby were made in the Report of the Imperial Wireless Telegraphy Committee, February 1924, presided over by Sir Robert Donald. An opportunity of meeting the recommendations came in the summer of 1924, when the Government, finding the majority of the Dominions would probably agree to use the Marconi beam system, made a contract with the company which will result in the establishment of duplex beam circuits between Canada and England, Australia and England, South Africa and England, and India and England. The arrangement promises a happy solution of many long-standing difficulties. These beam stations are now nearly completed. A transmitting station at Bodmin serves Canada and South Africa, and a station at Grimsby serves both Australia and India. Corresponding stations are being erected in the four Dominions. By the courtesy of the Marconi Company I am able to show lantern slides of some of these novel stations under construction. (Figs. 7 to 9 give a selection.)

The beam stations may claim to send their messages straight to the desired objective since the guaranteed angle of concentration of the waves is thirty degrees. There is the additional advantage that little power is consumed, partly because of the concentration and partly because short waves are used. This again implies small capital outlay, so at first it was thought that suitable beam stations would be very inexpensive. In practice the engineers met many snags, and it is probable that the English beam stations for communicating with four Dominions will cost altogether about half as much as Rugby. For this expenditure good communication is guaranteed for a daily average of 18 hours with Canada, 7 hours with Australia, 11 hours with South Africa, and 12 hours with India.

It has been suggested that beam stations may some day supersede the wireless stations which send their waves all round. This kind of controversy is always going on in wireless and other progressive sciences, and sometimes tends to cripple action to-day by promising cheaper alternatives to-morrow. As an example, when the motor-car gained its first successes we were told that railways were obsolete; but one can still travel by train from London to Edinburgh more comfortably, more cheaply, and more quickly than by car. As regards the wireless problem the present beam stations are the first of their kind and have not yet been operated. We only know enough to say that both systems will be useful, and that each can do something the other cannot.

Before the end of this year the long-range wireless transmitting equipment of the Empire will include—

(1) The multiple station at Rugby, comprising :—
  (*a*) Long wave plant of world-wide range.
  (*b*) Long wave plant of medium range.
  (*c*) Short wave plant of world-wide range.
  (*d*) An experimental telephony plant of range 4,000 miles.
(2) Medium power stations using long and short waves at Leafield, Oxfordshire, and at Abu Zabal, Egypt.
(3) Four beam circuits for duplex communication between England and the four largest Dominions.

Thus by the efforts of the engineers of the Post Office and the engineers of the Marconi Company the Empire will shortly possess the most up-to-date wireless equipment in the world.

So far I have dealt with things nearly completed, but now I must indicate briefly what has yet to be attempted and is within reach. In the first place there are important communities still unprovided with long distance wireless, for instance, New Zealand. This urgent necessity has been emphasised in the Report of the Donald committee. In the second place preparations ought to be made to utilise fully the new facilities. For instance, twice a day Rugby transmits a summary of news prepared by the Foreign Office, and conceivably other news services may be arranged. If I were a settler in Uganda or the owner of a remote sheep station in Australia, I should install a simple receiving set and so keep in touch with the world. But this implies learning the morse code, not as hard as learning the typewriter, but still a difficulty; and therefore in small communities where there is a local news sheet it would be more practical to organise a receiving service by paying an operator to listen regularly and decode the messages for the printer. In the Empire there are innumerable remote settlements to which the cabled news messages leak very slowly, and there are many which depend entirely for their news on the occasional visit of a ship. All such outposts can now by aid of a simple receiving set be made to feel that they are within five minutes of Fleet Street. Moreover, if these stations were registered messages could be specially addressed to them from Rugby just as telegrams are addressed to-day to ships on distant oceans.

Another method of distributing the morse messages from home when received, say, in New Zealand, is to announce them, after decoding, through the local broadcasting stations. In each of the Dominions such stations are growing in numbers, and by their aid messages could be distributed promptly to millions of listeners-in. Of course it may some day be possible, after further invention and development, for speech and music from England or other centre to

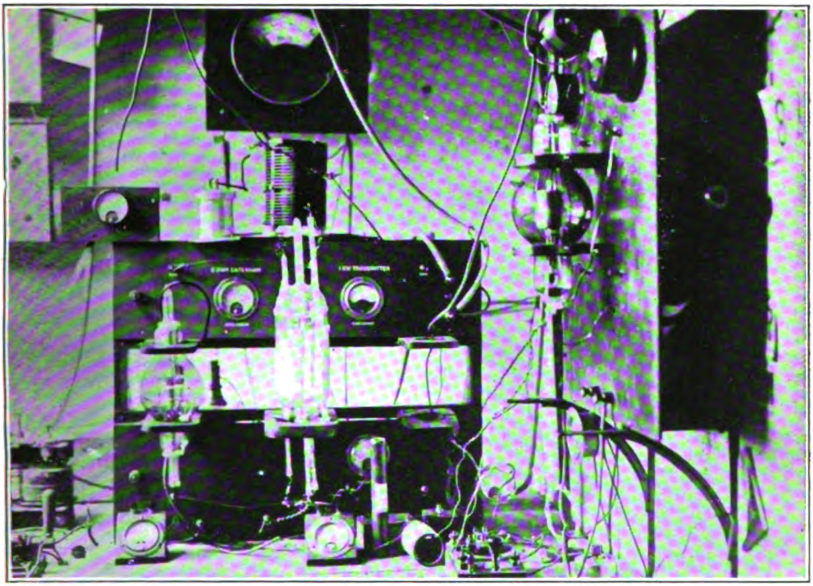

Fig. 10.—Mr. Marcuse's Transmitting Apparatus.

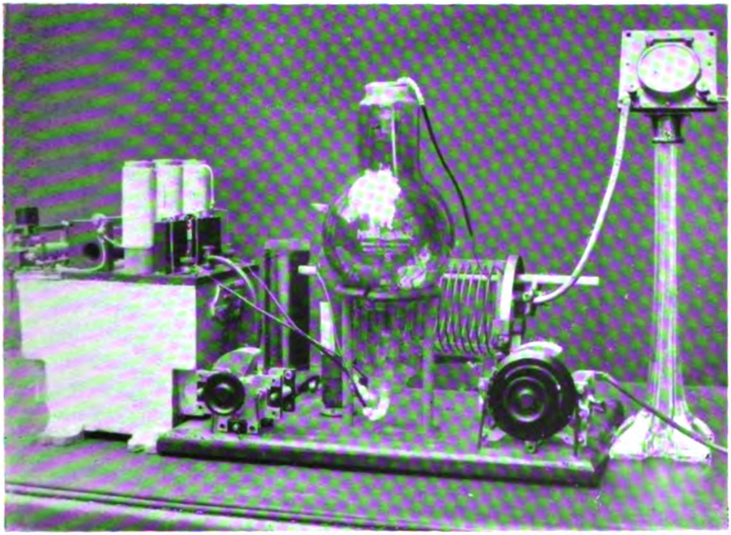

Fig. 11.—Mr. Simmonds's Transmitting Apparatus.

be put on to every local broadcasting station in the Empire; but I refrain from such prophecy and am now speaking of what can certainly be accomplished to-day by mere organisation of existing facilities.

But this does not exhaust the possibilities now arising. During the past two or three years wireless amateurs have succeeded in transmitting signals and even speech across vast distances with tiny apparatus. I have here some lantern slides (Figs. 10, 11) showing the apparatus used by Mr. Marcuse and Mr. Simmonds in talking with people similarly equipped on the Amazon and in New Zealand and elsewhere. The plant is so small that it could be installed in a drawing room. If only we could rely upon plant of this size getting through for days instead of hours then there would no need for high-power stations. Even so it is easy to imagine how such glimpses of inter-communication might be made use of in the elaboration of Empire wireless.

In concluding this Discourse, may I summarise the chief results expected from all these efforts to establish an Imperial wireless network? They can be put in two sentences. Firstly, mutual trade will be facilitated by rapid and cheapened communications, and finance will benefit by a shortening of credit such as followed the introduction of the cables. Secondly, the interchange of opinion between all sections of the Empire will become fuller; and just as wireless on the small scale has become, in the shape of broadcasting, an important social influence in Great Britain, so wireless on the large scale will have a similar influence in, and strengthen the unity of, our widespread Empire.

[W. H. E.]

ANNUAL MEETING,

Saturday, May 1, 1926.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1925, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Sixty-two new Members were elected in 1925.

Sixty-three Lectures and Twenty Evening Discourses were delivered in 1925.

The Books and Pamphlets presented in 1925 amounted to 254 volumes, making, with 567 volumes (including Periodicals bound) purchased by the Managers, a total of 821 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, K.G. C.B.E. M.V.O.
Treasurer—Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S.
Secretary—Sir Robert Robertson, K.B.E. M.A. F.R.S.

Managers.

Sir Harry Baldwin, C.V.O. M.R.C.S.
Sir John Biles, K.C.I.E. LL.D. M.Inst. C.E.
Sir James Crichton-Browne, M.D. LL.D. F R.S.
John Mitchell Bruce, C.V.O. M.D. LL.D.
The Rt. Hon. Viscount Chelmsford, P.C. G.C.M.G. C.B.E.
Ernest Clarke, C.V.O. M.D. F.R.C.S.
W. H. Eccles, D.Sc. F.R.S.
Sir Oliver Lodge, LL.D. D.Sc. F.R.S.
Charles H. Merz, M.I.E.E.
Sir Ernest Moon, K.C.B. K.C. LL.B.
Sir Edward Pollock, F.R.C.S.
Sir Arthur Schuster, D.Sc. LL.D. F.R.S.
Sir Henry Wood, Mus.Doc.
Sir Almroth Wright, K.B.E. C.B. M.D. F.R.S.
Sir Alfred Yarrow, Bart., F.R.S. M.Inst. C.E.

Visitors.

K. A. Wolfe Barry, M.Inst.C.E.
James H. Batty, M.R.I.
W. A. F. Balfour Browne, M.A. F.R.S.E.
Charles Carpenter, C.B.E. D.Sc. M.Inst. C.E.
Alfred Carpmael, B.A.
Charles J. P. Cave, M.A.
A. Chaston Chapman, F.R.S.
Alexander F. H. Dick, M.R.I.
W. Vaux Graham, M.Inst.C.E.
Sir Lawrence Jones, Bart. J.P. M.A.
Ronald Malcolm, M.R.I.
Sir Walter Peacock, K.C.V.O.
Hugh Munro Ross, B.A.
Frank Coston Taylor, M.A.
John Tennant, M.A.

GENERAL MONTHLY MEETING,

Monday, May 3, 1926.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Manager and Vice-President, in the Chair.

George Muirhead Clark,
Mrs. Samuel Courtauld,
Richard Maurice N. Dawlings,
Walter Hugh Frizell,
Charles Stanley Gibson, O.B.E.,
Sir Henry P. Maybury, K.C.M.G. C.B.,
Miss Lilian A. Quartly,
The Hon. Sir William Francis Kyffin Taylor,
K.B.E. K.C. J.P.,

were elected Members.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., etc., was elected Honorary Professor of Natural Philosophy in conformity with the Bye-Laws, Chap. XIX. Art. 2.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. F.R.S., etc., was elected Professor of Natural Philosophy in conformity with the Bye-Laws, Chap. XIX. Art. 2.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

National Illumination Committee of Great Britain—Commission Internationale de l'Eclairage. 1924. Recueil des Travaux. 8vo. 1926.
Rockefeller Institute for Medical Research—Studies, Vol. LV. 8vo. 1926.
Shirley Institute (the Director)—Shirley Institute Memoirs, Vol. IV. 8vo. 1925.

WEEKLY EVENING MEETING,

Friday, May 7, 1926.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Secretary and Vice-President, in the Chair.

SIR FREDERIC G. KENYON, G.B.E. K.C.B. F.B.A. D.Litt., Director and Principal Librarian, British Museum.

English Illuminated Manuscripts.

THE object of this address is to call attention to a department of English art which is often overlooked, and to make a claim which we seldom make for ourselves, and which consequently is rarely considered by other nations. It is a commonplace that we as a nation are addicted to self-depreciation. If this were due to modesty, there might be some merit in it; but it seldom is so. More often it is a critic's method of showing his superiority to local or natural prejudices, and the more catholic range of his taste. It is not a sense of his own inferiority that makes him confess the failures of his people: it is a lively sense of the inferiority of others. This is a less healthy temperament, and it has harmful effects. If a people is led to believe that it is incurably ignorant, or poor-spirited, or ineffective, or inartistic, it is apt to acquiesce in a lower range of achievement. It is satisfied with lower ideals than are in fact within its reach. It is always good to set one's ideals high, to believe that a great success is attainable if we try. "Possunt quia posse videntur."

Now there are fewer convictions more firmly rooted in our national self-consciousness, or more widely held with regard to us by foreigners, than that we are, as a nation, inartistic—lacking in artistic taste, and (with certain admitted exceptions) poor in artistic performance. We are apt to say so ourselves, and sometimes even take pride in it. Our orators, if they happen to know Latin, declaim the Virgilian tag, "Excudent alii," and I have heard a Cabinet Minister confess with complacency that he was a Philistine in matters of art and learning. Small wonder if foreigners take us at our word.

I cannot undertake to survey the whole artistic achievement of Englishmen, and to consider how it stands comparison with the work of other nations. I have neither time nor competence for such a task. But I wish to bring to your notice one branch of art in which, over a period of several centuries, English taste and English workmanship can stand comparison with any other country in Europe —are not infrequently at the summit of the achievement of their time, and are never far from the front rank. This is in the art of painting (usually regarded as one of the weaker sides of English culture), as shown in the illuminated manuscripts of the Middle Ages, from about the year 700 to about the year 1400.

It is necessary to confine the inquiry to painting as shown in illuminated manuscripts, not because there was no other painting, but because so little of it has survived. The artistic reputation of England has suffered terribly by the Reformation and the Puritan movement. Both in the second quarter of the sixteenth century and during the time of the Commonwealth an almost incredible destruction was wrought among the paintings—both wall-paintings and easel-pictures—which formerly adorned our English churches. The records of the destroyers remain, in which they report to their employers with fiendish glee the number of paintings which they have defaced; a bad pre-eminence being enjoyed by Mr. William Dowsing, who relates how in 1643–4 he destroyed (to quote two instances only) 200 pictures in one place in Suffolk and 150 in another, and by "Blue Dick of Thanet," who claims the glory of the destruction of Canterbury.

It is only from the manuscripts that one can now judge what was the quality of the art thus blindly destroyed. A few paintings, on walls or panels, survive; but neither in quantity nor, with a few exceptions, in quality do they do justice to the capabilities of English artists.

I propose, therefore, to show on the screen examples of some of the finest illuminated manuscripts, and to leave them to make their impression on you. They are taken, with a few exceptions, from manuscripts in the British Museum, which fortunately is well equipped with first-class examples of all periods. It is, of course, a serious drawback that you see them without their colours. To appreciate their full beauty you must visit the British Museum. Coloured facsimiles of good quality are now obtainable, even in so cheap a form as picture-postcards; but coloured slides, so far as my experience goes, are seldom satisfactory. What you can see is the art shown in the lines and patterns of the artists; for the colours, rich or delicate as the case may be, you must use your imagination. Fortunately in one important and characteristic branch of English work the merit is that which comes from beauty of line, with little help from colour.

It will be understood that the art of the illuminator is primarily decorative. Although, as you will see, illustration in the sense of the depicting of incident is not by any means always absent, it is only in the fifteenth century, when English art was in its decline, that miniatures became definitely pictorial. Before that time the art of illuminated MSS. appears in the designs of initials, borders, and sometimes of whole pages. It aims at doing honour to the texts—generally Biblical or liturgical—to which it is applied by surrounding them with beautiful patterns in gold or colours. It is not, for the most part, representative art, nor is it imaginative art, except so far as imagination is employed in the creation of the beauty of line and pattern and colour.

Our earliest English art has its foundations in Ireland. That extreme corner of Europe was, in the seventh century, the spot in which Christian zeal and Christian art burned most brightly. From Ireland Christianity was carried by missionaries into the south of Scotland, where it had its headquarters at Iona; and from Scotland it passed into Northumbria. The missionaries brought with them not only their religion but the art with which it was adorned; and the first art of England is consequently properly described as Anglo-Celtic. Celtic art was composed mainly of interlaced patterns, sometimes of simple lines interwoven with every elaboration of intricate interlacement, sometimes of grotesque animal forms which lent themselves to similar treatment. An exuberant fancy is shown in these designs, which are heightened by vivid colours, such as bright reds and yellows. In England a change comes over the spirit of the art. The restraint and refinement which are characteristic of all the best English art show themselves at once in the subduing of the exuberance of the Celtic fancy. The interlacements are as elaborate, but the colours are more subdued, and the grotesquenesses are fewer. Infinite pains are devoted to the fashioning of the patterns; but pale greens and mauves take the place of yellows and reds, and the sobriety of tone adds greatly to the charm of the decorated pages and initials.

The masterpiece of the Anglo-Celtic school is the famous Lindisfarne Gospels, produced shortly after the year 690 in honour of St. Cuthbert. The contrast between it and the Book of Kells, which is the masterpiece of the purely Celtic school, shows at once the affinity and the difference between the two schools. In pure decorative pattern, unassisted by the splendour of gold, and with no intermixture of figure-interest apart from the long-limbed birds which are worked into the design, I do not think this magnificent book has ever been equalled.

The surviving examples of Anglo-Celtic paintings are few, and a marked gap separates them from the next efflorescence of English art. The invasions of the Danes probably led to the destruction of many manuscripts, and certainly hindered artistic, and indeed all intellectual, culture in the north and east of England. The succession passed elsewhere. When Charlemagne, about the year 800, wished to revive art and learning in his country, it was to England that he looked for scholars, scribes, and artists. Alcuin of York, inheritor of the traditions of Bede, was the master of the King's schools and the director of Biblical studies in France; and the influence of the

Anglo-Celtic school of illumination is plainly seen in a very gorgeous group of Gospels produced in north-eastern France in the early part of the ninth century. That art, which included other elements which may be traced to Byzantine influences, may be properly described as Franco-Celtic; and it deserves mention here, not only as the direct descendant of the Northumbrian school, but as having in its turn influenced the revival of the art in England which came about in the tenth century.

The renaissance of English learning after the disastrous period of the Danish wars was due to Alfred; and it was in Alfred's capital of Winchester, though not in Alfred's reign, that the renaissance of art took place. Towards the end of the tenth century, under the impulse of Æthelwold, Bishop of Winchester, a great school of illumination arose in the two Winchester houses, St. Swithin's (the Cathedral), and the New Minster, later known as Hyde Abbey. This school dominated the art of southern England until the Conquest. The earliest example is the charter of foundation of Newminster in 966; the finest is the Benedictional of St. Æthelwold, now the property of the Duke of Devonshire, produced about 985. Both of these, and several others, were certainly produced at Winchester; but it is not likely that the style was confined to the two Winchester monasteries. One manuscript which is wholly of this school was certainly connected with Canterbury; another (a very fine example) was unquestionably produced for, and possibly at, Bury St. Edmund's. Outside the area thus indicated the style is not known.

The work of this Winchester or South-English school falls into two categories, though they are sometimes combined. The first, and perhaps more definitely Winchester, form consists of decorative frameworks composed of broad bands of gold intertwined with rich conventional foliage, which enclose the first words of each Gospel. These have an extremely rich and handsome effect, and as the style did not last long enough to become mechanical (the curse of decorative art), they are never dull, nor do they ever exceed the bounds of good taste. In at least one instance that has come down to us, silver takes the place of gold in the bars of the framework; in another, which is perhaps the latest, but not the least pleasing, example of the style, thin rods in colours are substituted for the broad bands of gold. It is a highly decorative style, and had no equal on the continent of Europe at the time.

The other class of South-English work at this period consisted of figure-drawing in outline, with little use of colour. In origin this style does not appear to be English; at least the earliest example, the famous Utrecht Psalter, can apparently be located in the north-east of France. It took root in England, however, where one copy of the Utrecht Psalter itself was produced in the ninth century (Harl. 603), and others (at rather further removes) later. It is found also in several books produced at Winchester, and in others which are certainly English but cannot be precisely located, sometimes by itself and sometimes combined with the gold bands and conventional foliage which have been described. A group of manuscripts of the poems of Prudentius contain some particularly pleasing examples of the style. It is notable alike for its grace and for its life. It is fond of fluttering drapery, which seems to be floating in a strong breeze. It depicts figures in motion with much liveliness. It is a reticent style, suggesting rather than completely representing, and it makes very sparing, if any, use of colour. Though apparently foreign in origin, it is typically English in development, and we shall find it reappearing at a later date in one of the most charming volumes ever produced in this or any other country.

The Norman Conquest brought English art, as well as English literature, to an end for a time. French influences were naturally supreme in the monasteries, and the modifications which appear, both in writings and in illumination, must be mainly attributed to this source. From the last third of the eleventh century there is little to show, and when art reappears in the twelfth century it is in a different form, which shows no trace of the Winchester style. The outline style may be seen in such work as the Guthlac Roll (in honour, it may be observed, of an English saint), and the elongated figures of certain MSS. relating to St. Edmund and others, though not in outline, recall in some measure the figures of the Utrecht Psalter and its descendants. But the main splendour of twelfth century work appears in elaborately decorative initials, in a style of which the origins can be traced to the Continent, and which manifested itself in France, Belgium, and Germany as well as in England. Its other principal characteristic is figure-drawings which show signs of strong Byzantine influence; and this also must have come from the Continent.

In the course of the twelfth century strong schools of miniature art were established at Bury St. Edmund's and St. Albans; while other important centres were at Canterbury, Winchester (where the Cathedral priory of St. Swithin's takes the lead of New Minster, which had changed its quarters and is now known as Hyde Abbey), and Durham. The volumes now produced were often of much greater size than had previously been usual. Thus we have from this century three enormous Bibles, all decorated with large historiated initials, the Pudsey Bible of Durham, the Bury Bible now in the library of Corpus Christi College at Cambridge, and the great Winchester Bible. To these may be added the Eadwine Psalter, produced at Canterbury, now at Trinity College, Cambridge. There is thus ample evidence of artistic activity in England, but it is less distinctively national than in the Anglo-Celtic and Winchester periods. The manuscripts of this period are, however, exceedingly handsome, and are further commended by their beautiful handwriting—perhaps on the whole the finest writing in any period of Latin minuscule palæography. The figure-drawing, also, if it has lost something of the life and vividness of the Wessex outline style, has gained in solidity and the representation of the human body. Some pages in the Winchester Bible, as well as the Guthlac Roll, testify to the continuance of the outline tradition.

When we pass to the thirteenth century a gradual change of style, in illumination as well as in handwriting, is evident, but England is still proceeding hand-in-hand with France. The general tendency is towards work on a smaller scale and with greater delicacy of workmanship, and the finest specimens, if less magnificent than the great Bibles of the twelfth century, are more beautiful in detail. Three classes of manuscripts may be particularly mentioned. The reign of St. Louis in France was marked by a great outburst of activity in the production of copies of the Bible. Under the stimulus of the University of Paris, there was a great output of Bibles in a very small format, evidently intended for private use rather than for the church lectern, written in tiny hands which must have greatly tried the eyesight of their readers, and decorated with small initials of very tasteful execution. A particular feature, which is almost universal, is a series of small miniatures of the days of Creation, decorating the first page of the Book of Genesis. This style spread quickly to England, and many copies were certainly produced in this country; but the style is so similar on both sides of the Channel that in default of express evidence it is often very difficult to determine whether a particular manuscript was produced in England or in France. Neither country can claim any pre-eminence in workmanship over the other.

A second class, which again is shared by both countries, is the remarkable group of illustrated copies of the Apocalypse, which begins in this century and goes on into the fourteenth. This was at one time claimed as specifically French, but the progress of research has invalidated this claim. Not only some of the finest, but also some of the earliest, are quite certainly of English origin. At the head of them stands the great Apocalypse of Trinity College, Cambridge, which there are good reasons for assigning to the scriptorium of St. Alban's. Others, which are also certainly English, are in the British Museum, the Bodleian, and the Lambeth Library. The illustrations are often grotesque in character, the symbolism of the Apocalypse lending itself easily to such treatment; but they also show much vigour in drawing and (especially in such examples as the Douce manuscript at Oxford and the Lambeth Library copy) much delicacy and refinement of line. The artists clearly revelled in the opportunity of indulging their fancy which the material of the book provided for them.

The third class, which also begins in this century and continues into the next, is composed of illustrated Psalters. Of these there is a series of great beauty, some English and some French. It is in these especially that we see the development of the border, which is at the root of the progress of the art of illumination during the next two centuries. The border is in principle a growth from the illuminated initial. The initial, beginning as a square decorative letter, puts out shoots which at first stretch a little way above and below the original rectangle, and gradually push further and further up and down the left-hand margin of the page or along the top, then turn the corners and (in the fourteenth century) press on until they meet and form a complete border enclosing the column of writing. These shoots are characterised by firmness and strength, accompanied by much delicacy of execution. They are floral in design, forming stems from which small leaves project, and, as the style develops, birds and beasts and men and grotesque figures are introduced, which perch on the shoots of foliage, and engage in miniature fights and other activities. For the combination of strength and grace the best of these manuscripts can hardly be surpassed. Some admirable examples are certainly of English workmanship, such as a Psalter belonging to the Duke of Rutland, and the famous Tenison Psalter, now in the British Museum, which was executed for Alphonso, son of Edward I. Other specimens of the same style, not in Psalters, are a Bible produced at Canterbury by William of Devon, and a copy of the Bible narrative of Petrus Comestor which Edmund, Duke of Cornwall, gave to the College founded by him at Ashridge.

One other group of thirteenth century manuscripts must be mentioned which is wholly English. This is a group of volumes produced at St. Alban's by, or under the immediate direction of, Matthew Paris, who was not only the greatest English historian of the Middle Ages but also an artist of unusual merit. In this we see the continuance of the outline tradition which had taken such deep root in England. Sometimes they are sketches of sacred subjects, such as the Madonna and Child; sometimes they are illustrations of Matthew's Chronicles of England. Always they are characterised by vigour and dramatic life, and have an individuality of style which is unusual in miniature work, where the individual is generally less recognisable than the school.

The thirteenth century was thus a period full of artistic life, with a high level of artistic achievement, but with no very strong line of demarcation (except in the St. Alban's school) between English and French work. In the fourteenth century the two schools diverge. Both continue at a very high level of excellence. Indeed, it may be said that at no other period in the whole history of the art of illumination were both simultaneously producing work of the first class; but they were developing on different lines. The best French MSS. of the first quarter of the century can hardly be excelled for grace and delicacy, both in the borders and in figure-drawing. They combine delicacy and strength, with a restraint which disappears in the more luxuriant work of the fifteenth century. But in richness and splendour they do not excel the contemporary work of English artists. At this period the leading English school had its home in East Anglia. The great houses of Norfolk, notably Gorleston, and perhaps Norwich, produced a school of surpassing beauty. The borders are composed of floriated designs of great richness, and combined with these are historiated initials of admirable execution. The favourite book was the Psalter. The Ormesby Psalter at Oxford; the Gorleston Psalter in the library of Mr. Dyson Perrins; the Douai Psalter (also produced at Gorleston), which was one of the victims of the Great War; the St. Omer Psalter, formerly in the collection of Mr. Yates Thompson, and now, by his gift, in the British Museum; the Windmill Psalter in the Pierpont Morgan library; and the imperfect de Lisle Psalter in the British Museum —are all works which stand in the first rank of illuminated art, at least equal in merit to anything that was being produced in France, though quite different in style. The colours are rich and harmonious, without gaudiness. The figures are full of life, and are greatly improved in drawing. The composition shows a fine decorative sense, with a lively and prolific fancy in details.

Yet even this splendid group of volumes does not reach the high-water mark of English art in this period. There is one book which stands apart on a pinnacle by itself. This is the celebrated (and yet not sufficiently well-known) Queen Mary's Psalter, so called because in the reign of Queen Mary it was confiscated by an active Customs House officer at Dover as it was being sent out of the country, and was by him forwarded to the Queen. Whence it came, or who were its previous owners, is not known; but in this way it entered the Royal Library, and so in due course passed with the rest of that library to the British Museum. As its name shows, it is a Psalter; but (as in some other English Psalters of the period) the text of the Psalms is preceded by a series of half-page pictures of Old Testament story, in this case executed in the most delicate style of outline drawing, which make it the summit and climax of this characteristically English form of art. The Psalter itself contains, at the beginning of each section of the book, initials, borders, and pictures of most delicate execution, more akin in style to contemporary French work than to that of the East Anglian school; but the lower margin of every page contains vignettes in outline work, very lightly tinted, containing delightful scenes of sacred legend, or of country life, or of grotesque and fanciful figures, which are among the most charming things in art. It is one of the most beautiful books in existence; and it is a pity that neither its artist nor its place of origin is known.

Another volume which deserves special mention, though not of equal beauty, is the Louterell Psalter, produced about 1340 for Sir Geoffrey Louterell, of Irnham, in Lincolnshire. Here the decoration in general has neither the taste nor the splendour of the best work of the East Anglian school; but the margins display an astonishing wealth of fancy in the invention of grotesque animals much larger in size than in any other manuscript, and brilliant in diversified colours; and along with these go lively representations of country life which are invaluable for our knowledge of the manners and customs of rural England.

By this time the short-lived East Anglian school was coming to its end. The French wars of Edward III., and still more the Black Death of 1348, diverted or destroyed the artistic life of the country. The patrons of art were drawn overseas, the artists themselves probably perished; and the tradition was not carried on. When English art revives again, half a century later, to a last outburst of productivity, the style is almost wholly different, and the centre of gravity had moved to another part of the country.

This last renaissance took place in the reign of Richard II. Its parentage is somewhat uncertain. It is not a direct continuation of the East Anglian school; for though its borders are of almost equal richness, the elements of which it is composed are different. Their most marked characteristic is the spoon- or cup-shaped foliage which luxuriates over the margins, replacing the more delicate ivy-patterns of the earlier style. The initials, in the finest examples of this period, are either large and elaborate decorative patterns, or else historiations which are in fact miniature pictures. There is some reason to believe that the style owes its origin, at least in part, to Bohemian artists who came over in the train of Richard's queen. Not that Bohemia can show any manuscripts of equal splendour, but there are certain resemblances in style, and in at least one conspicuous example German inscriptions, in a very minute hand, occur among the decorative patterns.

Two of the most splendid examples of this school, both now in the British Museum, appear to have been executed for the chapel of Richard himself. One is a huge Bible adorned with large initials, sometimes historiated, but oftener decorative, as well as with fine borders composed of narrow bands of gold or colours with luxuriant foliage. The other, a Missal, would have been an even more magnificent volume but for the cruel fate that befel it, apparently in the early part of the nineteenth century. The then owner of it conceived the unhappy idea that the beautiful initials would look more effective if they were separated from the text to which they belonged. Accordingly he set to work with his scissors and cut them all out, ruthlessly severing the branches of foliage projecting from them, and laid them down in two large albums: first, the historiated initials, then the largest decorative initials, then the smaller illuminated letters by successive gradations of size, grouping them in patterns and alphabets after the manner of the schoolboys of my earliest youth with crests. The letters themselves are of very great beauty. Those which are composed of decorative patterns are exactly like those of the companion Bible: while the historiated initials, which are more elaborate than those of the Bible, must have given this volume an even greater splendour. Among the figures is a portrait of a king, recognisable as Richard II. Where this magnificent pair of books was executed it is impossible to say—possibly they are the work of lay artists attached to the Court; but wherever and by whomsoever produced, they rank high in the records of the later period of English illuminated art.

Not quite the highest, however; for there is one stupendous volume which, as it is almost the last product of English illumination, is certainly also not far from being the finest. This is the great Sherborne Missal which for about a century has been the property of the Dukes of Northumberland. Though not quite as large as the Bible just described (24½ in. by 17 in.), it is still one of the largest manuscripts in existence, measuring 21 in. by 15 in., after some considerable mutilation by the binders; and it is also one of the most magnificent. It has further this unusual feature, that we know the name of the artist who created it. Over and over again throughout its pages occur two figures, often with names attached, one a Benedictine monk named John Whas (or Wass), the other a Dominican friar named John Siferwas; and inscriptions inform us that John Whas was the scribe and John Siferwas the artist of this great work. Other hands, it is true, appear in the latter part of the MS., and to another artist apparently must be attributed the credit for a remarkable series of English birds, delineated on a large scale, and with their English names attached, which forms an altogether exceptional feature of the book: but the most splendid pages are unquestionably the work of Siferwas.

Nor is this all that we know about this great artist; for in the British Museum is another MS., very imperfect, which has as its frontispiece a very large picture, showing John Siferwas presenting his book to his patron, Lord Lovel of Tichmersh (who died in 1408). It is some satisfaction to know so definitely the creator of this last, and almost greatest, production of the English school. We know also that the Missal was produced for an Abbot of Sherborne about the year 1400, and it is fair to assume that it was produced in that part of England, in all probability in the Abbey itself, of which the Benedictine John Whas may have been an inmate. The name Siferwas, or Cifrewast, was well known in the south-west of England at this time.

The Sherborne Missal (in this respect resembling the finest MS. ever produced in France, the Très Riches Heures of the Duc de Berri, the work of Pol de Limbourg and his brothers) is very individual in character. Illuminated manuscripts in general are the product of a school; this is the work of an individual artist. He delights in architectural designs, erecting great Gothic canopies over the figures of his saints in the margins. He is fond of birds, which perch on all the branches of his patterns. He has a peculiar type of dark-foliaged tree, which appears in the vignettes that decorate many of his margins. He makes admirable use of the fluttering draperies of angels; and in certain splendid pages he masses the plumage of cherubim and seraphim to gorgeous effect. English art, ending, as for all practical purposes it does, with the Sherborne Missal, ends certainly in a blaze of glory.

It is remarkable that so much splendour should have been followed by such swift extinction; but so it was. A few manuscripts of good quality can be assigned to the first decade of the fifteenth century; one, a Missal, was bequeathed by W. Melreth, an alderman of London, in 1446, but was probably executed earlier; and on the whole it may be said that the reign of Henry IV. saw the extinction of English illuminated art. The wars of Henry V. took many of the patrons of art overseas, and brought them into contact with the splendid school of miniaturists then flourishing under the patronage of Jean, duc de Berri. Hence when the Duke of Bedford desired a sumptuous Book of Hours for himself and his wife, it was to the artists of the duc de Berri that he had recourse; whence the splendid Bedford Book of Hours, which is now the finest monument of French illumination in the British Museum.

If there had ever been a chance of a revival of English art, it was destroyed by the Wars of the Roses, which almost destroyed the English nobility. When more settled times returned, Edward IV. could apparently find no native artists to work for him; at any rate we find that he commissioned a series of large Chronicles and other works from Flemish artists. Hence English art has no contemporary work to set beside the French school of the later fifteenth and early sixteenth century, with its rather tiresome repetition of the ivy-pattern border. The best work of this later period is to be found either in the exquisite little pictures of the Flemish school or the gorgeous colours of the Italian Renaissance. English art had ceased. Much of it was destroyed at the Reformation; and the very memory of what it had been for seven hundred years seems to have passed away, and has only been resuscitated in our own time by the study and enthusiasm of a group of scholars to whom gratitude is due from all who care for the artistic reputation of our country.

[F. G. K.]

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WEEKLY EVENING MEETING,

Friday, May 21, 1926.

Sir Robert Robinson, K.B.E. M.A. F.R.S., Secretary and Vice-President, in the Chair.

John E. Sears, Jun., C.B.E. M.A. M.I.Mech.E.

Recent Developments in the Art of Fine Measurement.

The subject of Metrology may perhaps more strictly be regarded as an art than as a science. At the same time, one cannot overlook the fundamental truth of Lord Kelvin's famous saying, that nothing is really known until one is able to measure it, nor the equally fundamental fact that all measurement, in the last resort, resolves itself into a determination of one or other of the three elementary quantities —mass, length or time. Metrology thus lies at the root of all quantitative science, and the relationship is reciprocal, for metrology in its turn makes use of every advance in science or in mechanical art which may serve to aid the attainment of greater accuracy in measurement.

In my talk to-night I am going to confine myself to one branch only of metrology—the measurement of length—in which, in spite of the antiquity of the subject, great advances have been achieved in comparatively modern times, and particularly in the last few decades. I shall commence with a short discussion of the question of the primary standards of length which serve to define the ultimate units of reference, comparison with which, either directly or indirectly, constitutes the process of measurement of any other length. There are, as a matter of fact, three competing forms of primary standard— the "end" bar, whose length is determined by the distance between its end surfaces; the "line" bar, whose length is determined by the distance between two suitable graduation marks ruled upon it; and the so-called "natural" standard, based upon some physical constant independent of any material representation, e.g. the length of the seconds pendulum. Each of these types in turn has at one time or another received legal recognition as a primary reference standard of length.

I have exhibited on the table upstairs a number of standards and models which will be of interest as indicating the progress that has been made. The earliest is the actual standard yard of Henry VII., an end bar of octagonal section, with roughly marked incisions representing inches up to one foot, and 1/16, 1/8, 1/4, 1/3 and 1/2 yard. It is worthy of note, as showing how well continuity has been preserved, that the length of this bar agrees within 0·04 inch with that of the present Imperial Standard Yard. On the other side of the picture we have the standard yard of Elizabeth, a square section bar which has been broken and crudely repaired by dowelling and binding the pieces together with two strips of sheet brass and copper wire. So far as can be judged, however, the length of the Elizabethan bar differed by no more than 0·01 inch from the present standard. It is astonishing to realise that this bar, in this condition, was actually the reference standard of England so late as 1824, when the first *Imperial* Standards were created. The new Imperial Standard Yard was generically a "line" standard, though the defining marks actually took the form of small dots instead of lines. It was laid down that the new standard, if ever lost or destroyed, should be replaced by reference to the "natural" standard of the seconds pendulum. The disaster thus provided for actually occurred unexpectedly soon, the new standard being destroyed in the fire in the Houses of Parliament in 1834. The Committee appointed to restore the lost standards soon found that it would not be possible to do so with as great accuracy by means of the seconds pendulum as by reference to other bars known to have been compared with the lost standard. The natural standard was therefore abandoned, and the new (and present) Imperial Standard Yard, a true line standard, was completed in 1845, and given legal status in 1856.

The history of the metre shows remarkable analogies. Originally defined by means of a "natural" standard, the earth's polar quadrant, it was soon found necessary to abandon this owing to lack of precision, and to rely on a material representation, the Mètre des Archives, an end standard in the form of a flat rectangular bar of platinum. This in its turn was followed by the present International Prototype Metre, a line bar of X-section, made of platinum-iridium, with the defining graduations on the neutral surface of the cross-section.

There can be no doubt that in the state of the art as it existed in their day those responsible for the construction of the present standards of the yard and metre acted rightly in choosing the material line standard as affording the greatest advantage in accuracy of comparison and measurement. The materials used were also well chosen from the point of view of constancy in length. Many copies of both bars were made at the same time as the originals, and distributed to the various countries of the world, and various of the copies have been re-compared with the originals from time to time. By the Weights and Measures Act of 1878 power was given to the Board of Trade to construct an additional bar as nearly as possible identical with the Imperial Standard Yard. The results of a number of comparisons of this later bar with a number of the earlier ones show that in the period of forty years which has elapsed since its completion it has shortened by about 0·0002 inch, and is now remaining constant, in relation to them. It is a reasonable deduction that the original bars themselves shortened by a similar amount in the forty years prior to the construction of the new bar, and have now steadied down to a constant value. There is nothing to indicate that any change has occurred in the absolute length of the present standard metre since its construction.

As regards accuracy of measurement, the lines on the yard bars, which are of earlier construction, are appreciably coarser than those on the metres. At the same time, it is worthy of record that the statutory decennial intercomparisons of the Imperial Standard Yard with its Parliamentary Copies in 1912 and 1922 exhibited no discrepancy exceeding 0·00002 inch, about one part in two millions of the total length. It has usually been assumed that with the better lines on the metre bars appreciably higher accuracy was attainable. Recent experience, however, has shown that it is not possible to get agreement of interpretation between different observers better than about 0·00001 inch.

This figure, therefore, sets a criterion for any new development in the matter of reference standards, and it may safely be said that this degree of accuracy could be surpassed at the present day, either by a material end standard with ends finished optically flat and parallel by means recently developed, or by a natural standard based on the wave-length of light. A material representation of the standard is, of course, essential for all purposes of practical measurements, and for this an end standard is more readily determined than a line standard in terms of the wave-length, which, as an ultimate reference unit, has the two-fold advantage of being reproducible in any part of the world without risks attendant upon the transportation of material standards, and also of being reproducible at long intervals of time without risk of change; and at their last meeting in 1923 the International Committee of Weights and Measures adopted in principle the eventual supersession of the present material prototype metre in favour of a wave-length standard. The national laboratories of the world are at present engaged on the investigations necessary to the formulation of a suitably controlled system based on this principle.

I commence by a description of the methods of construction and measurement of the new end standards, one of which is included in the exhibit upstairs.

These bars are produced by a method developed some seven years ago at the National Physical Laboratory by my then colleague, Mr. A. J. C. Brookes, and they are now being manufactured commercially by The Pitter Gauge and Precision Tool Company, Woolwich, under patents held by him. Some of them are on the table. The process is extremely simple. The bar, which is of circular section, is finished approximately to size, and is then mounted with its axis vertical in a special lapping jig (Fig. 1) above an accurately flat circular horizontal surface plate, with its under surface in the form of an annulus exactly parallel to the upper side. The base plate on which this annulus rests is adjustable, and by placing a sensitive level on the surface plate and adjusting until it can be rotated without alteration to the indication of the level, the upper surface is brought exactly horizontal. The bar to be finished is held at its two ends in adjustable clamps, in which it is capable of rotation. A sensitive level is attached temporarily to the bar, and the clamps in turn are adjusted until it can be rotated without alteration in the indication of the level. Its axis is then exactly vertical. A flat parallel lapping block is then worked between the surface plate and the end of the bar, with the result that its end is finished accurately flat, and exactly square to its axis. The bar is then turned upside down, and the other end finished in the same way, with the final result that the two ends are rendered both flat and parallel to each other with great precision. It has been possible in this way to produce bars with highly polished ends, both flat and parallel, within one or two millionths of an inch over the whole of a surface 7/8 inch in diameter, and with no errors in length exceeding the possible errors of comparison with the present-day line standards.

That great pioneer of industrial measurement, Sir Joseph Whitworth, who introduced the first end standards with any pretence to accuracy, also designed and constructed, in addition to the well-known Whitworth measuring machine, a special measuring machine nominally capable of comparing two bars to the accuracy of a millionth of an inch. It may be taken for granted, however, that not only were the bars of those days insufficiently regular in form for measurement to this degree of accuracy to be possible, but the principle of Whitworth's "millionth" machine was not such as to afford any real prospect of actual measurement to this accuracy. The next important advance in accuracy after Whitworth was due to Mr. C. E. Johansson, of Eskilstuna, Sweden, who in 1908 introduced the now well-known Johansson gauges. These consist of blocks or slips of hardened steel, with flat parallel end faces finished to such a high degree of accuracy that when perfectly clean they will adhere together by "wringing," without any magnetic or other attractive force. These gauges, which are arranged in series of appropriate sizes for the purpose, may be wrung together in groups so as to make up any desired size to the nearest ten-thousandth of an inch. To do this involves an accuracy of the order of 0·00001 inch in the individual gauges, which again demands the possibility of measurement, for the purpose of standardisation, to an accuracy of about 0·000001 inch.

I have some of these gauges on the table, and can demonstrate the process of wringing, and the power with which they adhere. The nature of this wringing process has recently been investigated at the N.P.L., and is of considerable interest. It appears that if the gauges are perfectly dry they will not wring, and that the presence of a minute quantity of grease or moisture distributed as a thin film between the gauges is necessary to produce the effect.

Two methods have proved practicable for determining the thickness of these films, which obviously might be a matter of some importance in the use of the gauges. In one method a tiny drop of oil of known density, formed on the end of a fine wire, was used to produce a wringing film between a steel surface and a glass proof plane. A number of similar drops were weighed so that the volume of the drop was known, and the area which it occupied when the wringing was completed was easily measured under a microscope, so that its final thickness could be calculated.

It is of interest to note that the spreading of the liquid took some considerable time (in some cases several hours), depending on the viscosity, before the final condition of wringing was reached. The final thickness, however, was in all cases found to be of the order of half-a-millionth of a centimetre. This result has also been confirmed by measurement of the same gauges individually, and when wrung together in groups, by the optical interference method.

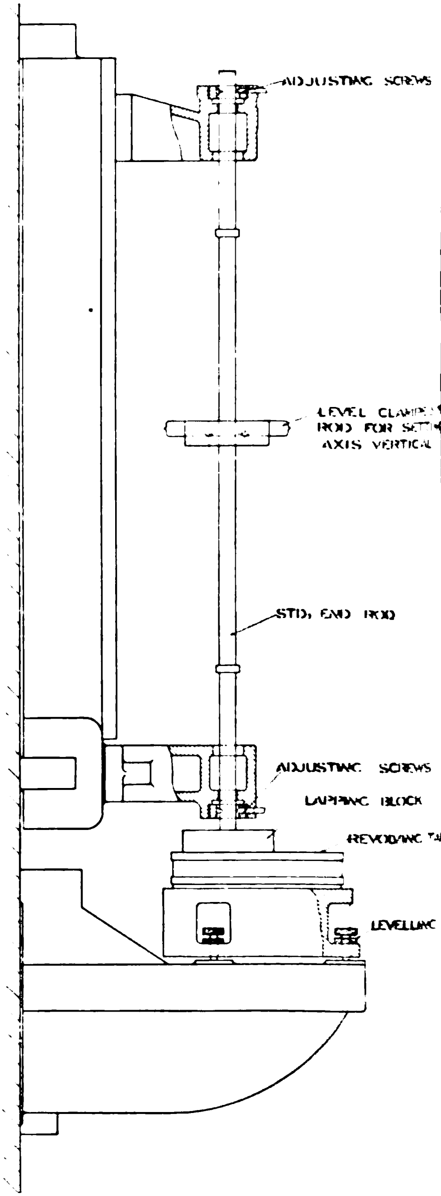


Fig. 1.—Lapping Jig.

It is remarkable to notice that it corresponds to the minimum thickness of a liquid film in the free state—the black area of a soap film—two, or possibly three, molecules thick, and one may perhaps conclude that the strength of the wringing action is attributable to very short "chains" of the liquid molecules attached at their ends to the two solid surfaces.

The actual thickness of the film is not of sufficient importance to be material in the industrial use of the gauges, and in the ordinary process of mechanical calibration it is, as a matter of fact, automatically allowed for, the determined size of each gauge being mathematically associated with one film thickness, which may be imagined to be distributed one half on either end of the gauge, so that when two gauges are wrung together a complete film is produced between them.

The method of producing these shorter end gauges is also a matter of considerable interest. The original Swedish maker has never disclosed his process, but during the war there was a great demand for these gauges, and at one time it appeared that supplies might fail. With a view to meeting the demand, experiments were made with a very simple method evolved by Mr. Brookes and myself at the N.P.L., which proved so successful that arrangements were being made by the Ministry of Munitions for their production on a considerable scale when the armistice was declared. The manufacturing rights have now been taken over by The Pitter Gauge and Precision Tool Company, Woolwich. The gauges are made by hand in groups of eight, of identical size, at a time. They are first rough ground to size, and then placed on a magnetic chuck, with eight facets arranged as shown in Fig. 2. The facets of the chuck are first lapped upon a flat lapping plate until they are all accurately flat and co-planar, the flatness of the lapping plates being attained in the first instance by working them together, in groups of three, on the well-known principle laid down by Sir Joseph Whitworth for adjusting surface plates. The exposed surfaces of the eight gauges are then similarly lapped until they are all flat and co-planar. They are then turned over and re-arranged in the manner shown in Fig. 3 The numbers in this figure may be taken as representative of the thicknesses of the various gauges at the points indicated, and it is easy to show that after the interchange the average thickness of the pair of gauges at each corner of the lapping chuck is the same. This is rigorously and mathematically exact.

The newly exposed surfaces are now lapped in turn until they are also flat and co-planar, and it follows, if the rate of lapping is uniform, that the thickness of the gauges at each corner must now be identical and their surfaces therefore parallel. Lapping is then continued, repeating the interchange at intervals to maintain parallelism, until the desired thickness is reached. Since all the gauges are identical in thickness, this is readily checked by wringing all eight of them together, comparing with a single reference gauge of eight times the desired size.

One very simple way in which this can be done is as follows:—The reference gauge, and the group to be tested, are wrung down side by side on a small surface plate, and a straight edge drawn carefully across the exposed surfaces. A slight mark can then be made across the full width of the higher of the exposed surfaces, while the lower surface will only show a mark at its outer edge. A difference of only 0·00001 inch can be detected in this way, corresponding to a little over 0·000001 inch on the thickness of any individual gauge in the group of eight.

Since the advent of these gauges has provided objects of sufficient perfection to be really capable of being measured to this order of accuracy, mechanical means have also been developed for achieving the same result. In any machine intended to work to such accuracy it is absolutely essential to eliminate any possibility of friction or backlash. The ordinary types of slides and pivots must be avoided, as also the use of a screw and nut with its attendant possibilities of error. The whole machine, in fact, must be used simply as a comparator, to determine the small residual differences between pieces, or combinations of pieces, of nominally equal length. Thus, for example, suppose we have a series of gauges of nominal values 1″, 0·9″, 0·8″ . . . 0·1″, and wish to determine the values of all the smaller gauges in terms of the 1″. We compare the 1 inch in

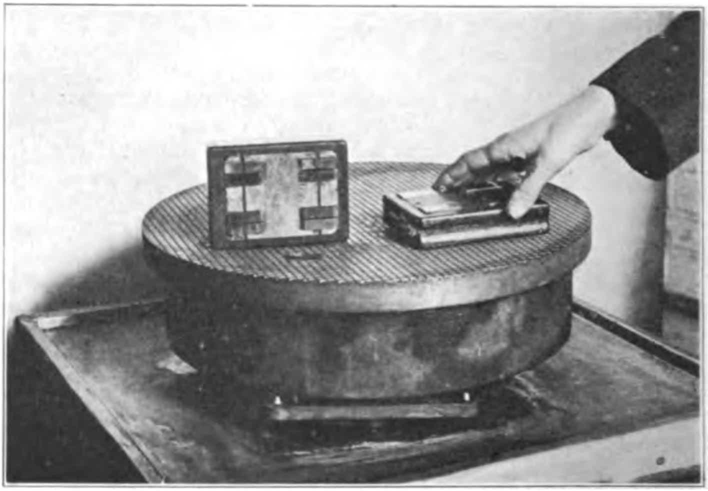


FIG. 2.—LAPPING SLIP GAUGES.

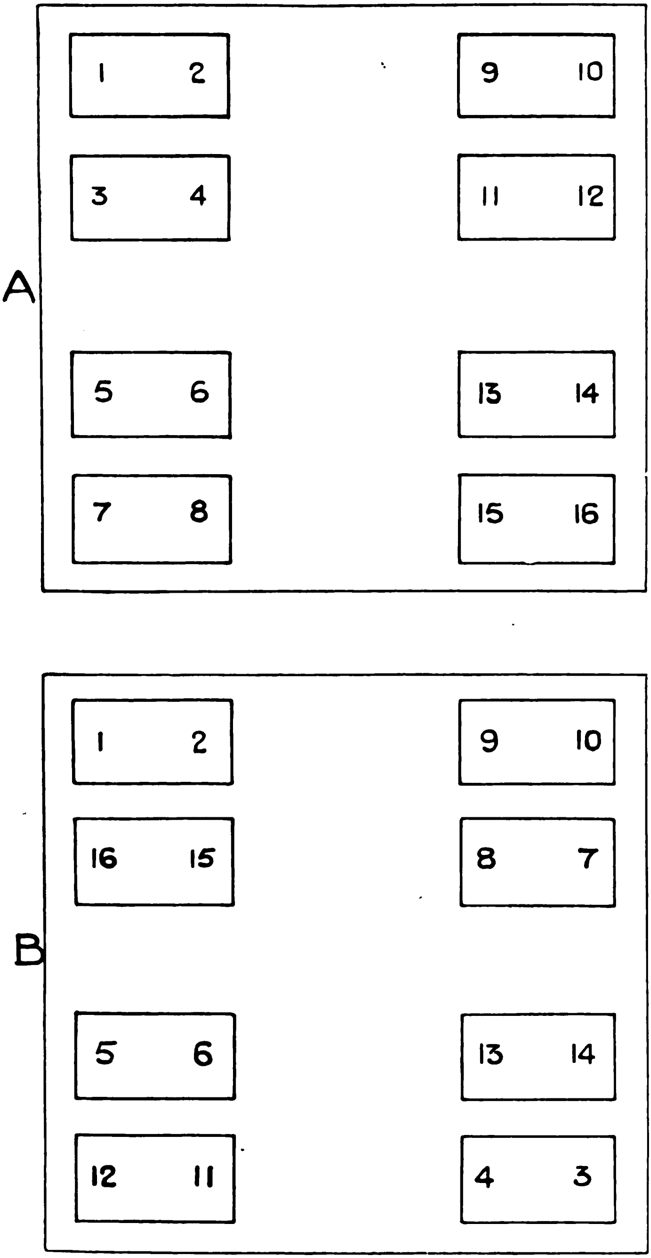


FIG. 8.—SCHEME OF INTERCHANGE IN LAPPING SLIP GAUGES.

turn with the sums of 0·9″ and 0·1″, 0·8″ and 0·2″, 0·7″ and 0·3″, 0·6″ and 0·4″ . . ., and so on. It is convenient, for the sake of symmetry in the work, to have a duplicate set, each unit of which can be directly compared with the corresponding member of the set under verification. We can then complete the above set of comparisons with 1 inch against the two 0·5 inches, and thus, in effect, with twice the 0·5″ of the principal set. Other similar series of comparisons are possible, and we may set out the whole of the comparisons we can make as follows :—

TABLE.

1+·9									
1+·8	·9+·9								
1+·7	·9+·8	·8+·9							
1+·6	·9+·7	·8+·8	·7+·9						
1+·5	·9+·6	·8+·7	·7+·8	·6+·9					
1+·4	·9+·5	·8+·6	·7+·7	·6+·8	·5+·9				
1+·3	·9+·4	·8+·5	·7+·6	6+·7	·5+·8	·4+·9			
1+·2	·9+·3	·8+·4	·7+·5	·6+·6	·5+·7	·4+·8	·3+·9		
1+·1	·9+·2	·8+·3	·7+·4	·6+·5	·5+·6	·4+·7	·3+·8	·2+·9	
1	·9+·1	·8+·2	·7+·3	·6+·4	·5+·5	·4+·6	·3+·7	·2+·8	·1+·9
·9	·9	·8+·1	·7+·2	·6+·3	·5+·4	·4+·5	·3+·6	·2+·7	·1+·8
·8		·8	·7+·1	·6+·2	·5+·3	·4+·4	·3+·5	·2+·6	·1+·7
·7			·7	·6+·1	·5+·2	·4+·3	·3+·4	·2+·5	·1+·6
·6				·6	·5+·1	·4+·2	·3+·3	·2+·4	·1+·5
·5					·5	·4+·1	·3+·2	·2+·3	·1+·4
·4						·4	·3+·1	·2+·2	·1+·3
3							·3	·2+·1	·1+·2
·2								·2	·1+·1
·1									·1

Where each row contains all the possible combinations of any one nominal size, which may be compared together.

It will be noticed that each column after the first contains one gauge which is common to all the combinations in that column. And if we take the whole of the comparisons between the combinations of any such column (say that containing the 0·4 inch gauge) with the corresponding combinations in the first column, and add the results, the sum of all the gauges from 0·9″ to 0·1″ cancels out, and we get finally a comparison between ten times the 0·4 inch gauge and four times the 1 inch, whereby the 0·4 inch is determined in relation to the 1 inch, as required, without measuring by means of the machine any quantity exceeding the small residual errors between combinations nominally identical.

I have on the table two entirely different machines, each capable of mechanical measurement of such small differences to an accuracy of 0·000001 inch. In the first machine friction is overcome by mounting the moving member on two parallel spring steel strips, which constrain it to remain always parallel to itself. The movement is indicated by means of a light finger also attached by two parallel steel strips, one to the fixed base of the machine, and one to the moving member, in such a way that when the latter is displaced the bending of the strips causes the finger to rotate. The finger carries at its end a fine wire stretched across a small ring, and the image of this wire is projected optically on to a scale. In ordinary use a magnification of about 20,000 to 1 is employed, so that 0·000001 inch appears as 0·02 inch on the scale. For the purposes of this lecture, however, I have succeeded in increasing the magnification about 5 times more. I do not expect that you will be able to see the movement corresponding to 0·000001 inch from the auditorium, but I have hope that a movement of 0·00001 inch, which now corresponds to 1 inch on the screen, may be readily detected. If I put in two gauges differing by 0·0001 inch I have no doubt that you will be able to observe the difference.

By way of visualising the magnitude of the quantities with which we are dealing, it may be worth while to remark that it takes about a thousand ordinary cigarette papers to make up 1 inch, and that the thickness of each of these must be again divided into a thousand equal parts in order to realise the millionth of an inch.

The second machine works on an entirely different principle, and is more particularly suitable for comparing combinations of greater length. The long end bars which I described earlier in the evening have also the property of wringing together, and comparisons and calibrations of such bars can be carried out with this machine in the same manner as shorter gauges are dealt with by the other. It consists essentially of two parts: a rotating horizontal surface plate similar to that used in the construction of the gauges, and a sensitive level mounted in a frame beneath which are two ball feet. The two gauges (or combinations) to be compared are wrung down side by side on to the surface plate, and the level, by means of an adjustable cradle in which it is carried, is brought over the gauges, and lowered gently into contact with them so that one ball foot rests on each gauge. The level is now released from the cradle, so that it is entirely supported by the gauges. The indication of the bubble is read, and the level lifted out of contact. The rotating table with the gauges is turned through 180°, and the level lowered into position again, the new indication of the bubble being also observed.

The difference between the two readings gives the measure of the difference in length between the two gauges. With a level of 833 ft. (10,000 inches) radius the magnification is again 20,000 to 1, as in the other machine, and the millionth of an inch can readily be detected.

Two great advantages of this instrument are that as its indications depend solely on gravity, they are independent of the rigidity of any part other than the base supporting the turntable, and that as the bars stand side by side in close proximity, and are never touched during the observations, temperature disturbances are minimised. With long bars temperature variation is, of course, a more serious difficulty than with the shorter gauges.

Having considered mechanical methods of measuring these accurate end gauges, we now come to the methods of optical control. Perhaps one of the simplest cases of optical interference, and one having a direct application to the problem in hand, is that which occurs when a pencil of light passes across a space bounded by two flat and parallel optical surfaces of semi-silvered glass, as shown in Fig. 4. The full line shows the progressive light wave travelling continuously through the system. A part of the light, however, will be reflected at the surface A, returning along the path indicated by the dotted line, only to be again reflected by the surface B, whence it once more travels forward in the path indicated by the dot-and-dash line, finally emerging together with the non-reflected portion of the original ray. The relationship between the reflected and non-reflected portions after emergence will depend upon the distance between the plates. The reflected ray traverses this distance twice, so that if the distance is an exact multiple of half a wave-length the two portions will be in phase, and the field will appear bright. If, on the other hand, the distance is an odd multiple of a quarter wave-length, the emergent portions will be in opposition, and darkness will result.

Suppose now that the system is traversed by a composite beam, consisting of a bundle of slightly convergent or divergent rays. The difference in the distance travelled by the two portions of any individual ray will depend on its inclination to the surfaces, and will be equal to $2l \cos \theta$, where θ is the angle that the ray makes within their common normal. As θ varies this distance will pass through a succession of values alternately odd and even multiples of half a wave-length. Thus the field produced will consist of a series of concentric light and dark circles, each formed by rays making a definite angle with this normal. The effective image is in fact formed at infinity, and by measuring the angular diameters of the rings it is possible to deduce the distance between the surfaces in terms of the wave-length of the light used.

It will readily be appreciated that direct measurement in this manner would not hold the prospect of any very high accuracy in the result. The method, however, carries with it one very remarkable advantage, which enables such accuracy to be attained. When an *approximate* value has been obtained for the whole number of wave-lengths contained in the distance between the plates, the measurement of the angular diameters of the rings enables the fractional excess over and above the whole number of wave-lengths to be determined with precision. If the operation is then repeated with several different wave-lengths in turn, whose ratios are exactly known, it is soon found that the series of fractional excesses thus determined can only be associated with one series of *corresponding* whole numbers of wave-lengths, so that in this indirect fashion the

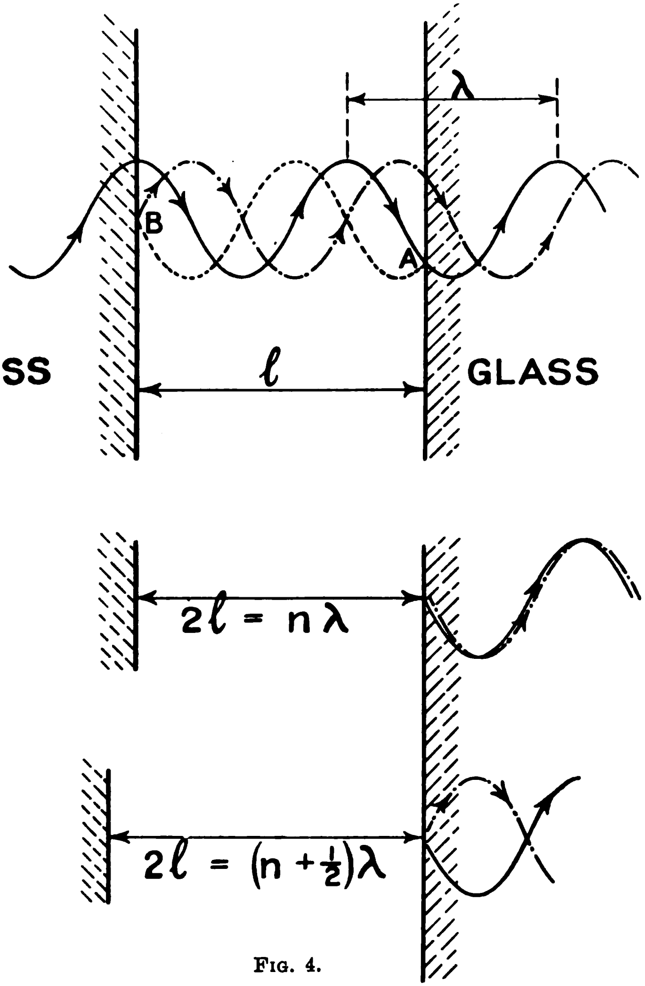


Fig. 4.

whole number is in turn exactly ascertained, and the distance between the plates is thus finally determined to a small fraction of a wave-length.

I have arranged an experiment here by which it is possible to project the interference patterns formed in this manner directly on to a screen. Using a mercury arc, you will see first how the rings expand as the distance between the plates is slowly increased. The rings corresponding to different wave-lengths expand at different rates, so that the colour scheme is continually changing. If I now cover the two halves of the field with colour filters, one green and one yellow, you will see the process more clearly, and you will also see that periodically the two series of rings come into phase, each successive coincidence corresponding to the passage of 17 yellow rings and 18 green ones, these numbers representing approximately the inverse ratio of the wave-lengths. The yellow rings as a matter of fact are themselves duplex, being formed by two lines of slightly different wave-lengths, and those of you who have good sight will be able to see, as I continue to separate the plates, that the pattern on the yellow side is slowly changing, and eventually, when the interference patterns due to the two wave-lengths get out of phase, a double set of rings appears, to be replaced in turn, on still further separation of the plates, by renewed coincidence. The increase of separation corresponding to successive coincidences in this case amounts to approximately 275 wave-lengths, and you will see that by observing coincidences in different wave-lengths in this way a very ready means is available of determining approximately the total number of wave-lengths by which the plates are separated at any stage. It has been found practicable to make direct measurements in this way of the lengths of parallel flat ended gauges up to 4 inches long by "wringing" glass proof planes on to their surfaces, and determining the optical separation between the exposed portions of the planes in terms of wave-lengths.

Since the distance between the successive rings in ordinary green light corresponds to about 0·00001 inch separation of the plates—and it is easily possible to measure to one-tenth of this—an accuracy of a millionth part of an inch is readily attainable. On a yard or metre bar this would correspond to about one part in forty millions, or about ten times the accuracy at present attainable with the existing line standards. It must be realised, however, that it is not possible to measure so great a length as this directly by the simple process just described. The maximum path difference over which it has hitherto been found possible to obtain suitable interferences is about eight inches, corresponding to a separation of four inches between the plates. It is, however, possible, once a standard of this length has been determined, to compare it stage by stage with similar standards, each having a length closely double or treble that of the one previously determined, by measuring the difference in path of light twice or thrice internally reflected in the shorter standard, and once internally reflected in the longer. This part of the work is done by means of white light, using some form of compensation method to equalise the two paths.

A determination of the length of the metre made in this manner by Fabry, Perot and Benoit, in terms of the wave-length of cadmium red, confirmed within one part in fifteen millions a previous determination by a somewhat different method by Michelson and Benoit in 1894. The very close agreement, in view of the difficulties of the final comparison with the line standard, must be regarded as partly fortuitous, but is none the less strong evidence for the reliability of the wave-length standard.

The introduction of the improved end standards to which I made reference previously will eliminate this difficulty in future by enabling direct comparison to be made by the interference method between the material standard and the optical reference, and will at the same time facilitate the attainment of the improved temperature control which is essential if that advance in accuracy, which is otherwise theoretically possible by the new method, is actually to be attained in practice.

If such an end bar is placed between the two semi-silvered plates of an intermediary optical standard such as I have described, the small spaces between its ends and the surfaces of the plates can be measured by direct interference in terms of the wave-length of light, and the final determination of the material standard in terms of wave-length is thus effected. It may be mentioned also that the same method of construction which is used for the end bars can be employed to make tubular intermediary standards with plane-parallel ends, and when the optical flats are wrung on to the ends of these tubes the whole can be evacuated, with the result that the wave-length measurements can all be made *in vacuo*, thus eliminating a number of difficulties arising from variations in atmospheric conditions when they are made in air.

Before passing from this subject reference should also be made to the experiments of Perard at the Bureau International des Poids et Mesures on the use of an end standard of crystal quartz. Such a standard being transparent and homogeneous, itself serves directly for optical measurement in terms of wave-length, and at the same time, from the very nature and antiquity of its material, holds strong promise of secular constancy. Its use, however, is limited to a standard not more than about 4 inches in length.

Whatever primary standard of reference may be chosen, both end standards and line standards are, of course, necessary for practical use, and while, as I suggested before, it may be found preferable to substitute an end bar for a line bar as the principal material standard, by reason of the advantage of direct comparison with the optical reference, and the increased definiteness associated with the new type of end bar, it will be no less necessary on that account to be able to make accurate comparison of line with end standards. This has always hitherto been a matter of some difficulty, but a new method has now been developed, also by a former colleague of mine at the

N.P.L., Mr. H. L. P. Jolly, which appears to impose no greater limitation of accuracy than is inherent in the nature of the line standard itself. This method involves the use of an intermediary bar B somewhat shorter than the two standards to be compared, together with two auxiliary end pieces, as shown in Fig. 5. These latter are carefully finished with flat and parallel ends, and are of lengths nominally equal to the difference between the intermediary bar and the standards being compared. Their thickness is equal to the radius of the intermediary bar, and their width equal to its diameter. Each has a graduation mark ruled approximately at the centre of its upper polished face.

The composite bar is first put together in the form shown in (1), Fig. 5, and compared with the line standard L in an ordinary comparator.

The result may be written

$$L = B + a_2 + b_1 + 2t + x_1$$

where t is the thickness of the wringing film.

Each of the end pieces is reversed in turn and the comparison repeated. We get a series of four similar equations, from which finally

$$L = B + \tfrac{1}{2}(a_1 + a_2 + b_1 + b_2) + 2t + \tfrac{1}{4}(x_1 + x_2 + x_3 + x_4) \ldots\ldots (1)$$

We then wring one piece at a time on to B, in the manner shown in (2), Fig. 5, and compare with E in an end-measuring machine.

We thus get

$$E = B + a_1 + a_2 + t + y_1$$

With the other end piece we similarly get

$$E = B + b_1 + b_2 + t + y_2$$

and hence

$$E = B + \tfrac{1}{2}(a_1 + a_2 + b_1 + b_2) + t + \tfrac{1}{2}(y_1 + y_2) \ldots\ldots\ldots (2)$$

Subtracting equations (1) and (2) we get

$$L = (E + t) + \tfrac{1}{4}(x_1 + x_2 + x_3 + x_4) - \tfrac{1}{2}(y_1 + y_2)$$

giving the desired comparison between L and E. It is to be noted that the lengths of the intermediary bar, and of the end pieces, are entirely eliminated, so that their exact values do not matter so long as they are sufficiently close to enable the various comparisons to be made with accuracy. The length of the end bar E is again associated with the thickness of one wringing film, corresponding to the mode of its actual use. The results obtainable by this new method (which again depends on the accuracy of flatness and parallelism of the new end standards and gauge pieces) are undoubtedly superior to those obtainable by any method previously available for the same purpose, and recent determinations made in this way at the N.P.L. have been remarkably self-consistent.

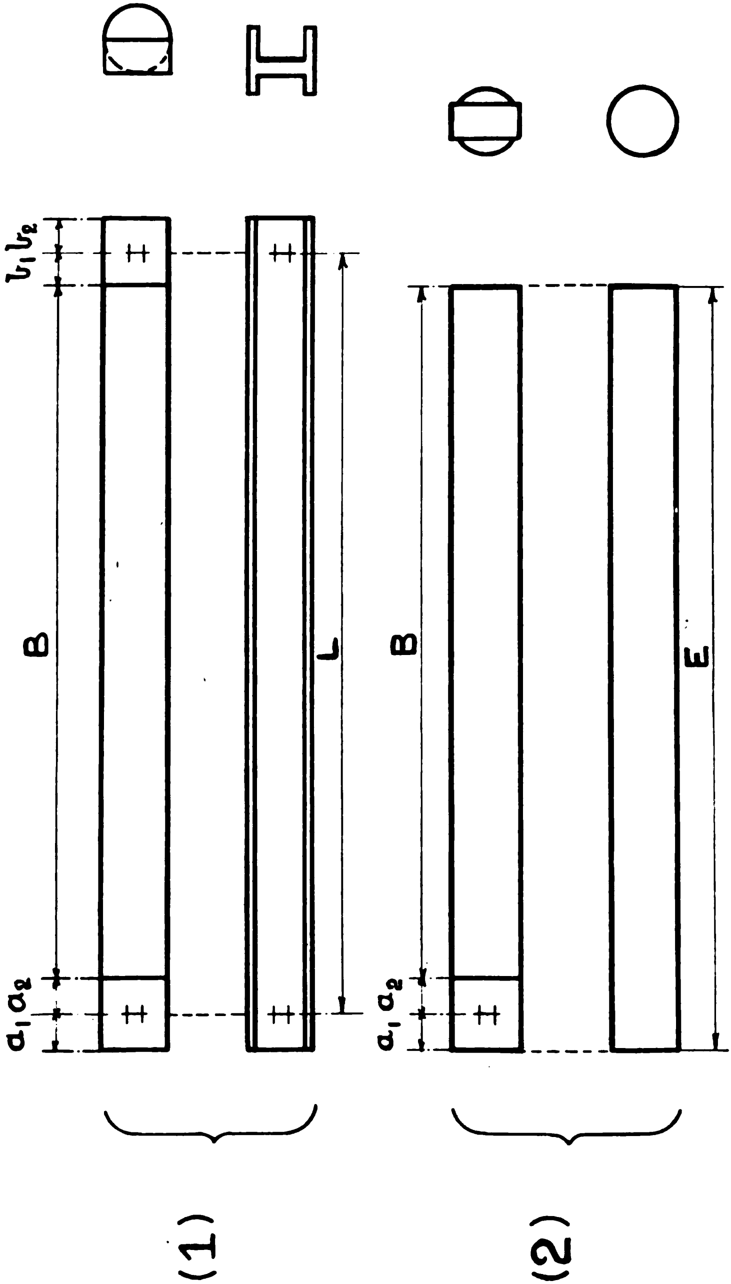


Fig. 5.—Comparison of Line with End Standards.

Before passing on to the last part of my discourse it may perhaps be of interest to mention the applications which have been made by Dowding, Whiddington and Thomas of the triode valve to the measurement of very small displacements. These methods have not yet provided any means of making absolute measurements, even of small objects, nor of determining small differences in length between larger objects nominally equal. And from the very nature of the methods it is difficult to see how they could be adapted to these ends. But for the measurement of small displacements of the same object they afford extreme sensitivity of reading; I have not time to describe them in detail. I have, however, a slide showing in simple diagrammatic form the principle of Mr. Thomas's arrangement, and he has been kind enough to arrange an experimental demonstration. The circuit used is known as the Hartley oscillating circuit (Fig. 6), and the action depends on the variation in tuning caused by the movement, in relation to the fixed inductance C, of the plate A attached to the object whose displacement is to be measured. The anode current *Ia* through the valve V passes through a sensitive galvanometer G, its average value being balanced out by the small battery and potentiometer circuit B, so that the galvanometer records the variations only of the anode current. With suitable values for the various components of the circuit, the law relating anode current and displacement may be made linear over a range of about 0·05 inch, and the movement of the galvanometer spot may be made to show a magnification of as much as 4×10^7 times, as compared with the original displacement. Lest such a method, and such very high magnification, should be regarded by some as being rather in the nature of an academic *tour de force*, I may mention that an instrument depending on this principle has been successfully used for such a practical engineering problem as determining the deformations of constructional members in bridges when subject to varying loads.

I now pass to a different phase of my subject. Another direction in which an important advance has been made in recent years is in the measurement of internal diameters. Previously the operation of passing from external to internal measurement had always been one of considerable difficulty. Two machines have, however, been designed within the past ten years, one by my colleague Mr. G. A. Tomlinson, and one by myself, which enable the operation to be performed with accuracy, and which, working on totally different principles, give results consistent to the order of 0·00001 inch. These machines are not well suited for demonstration to a large audience, but I will endeavour to explain the principle of one of them by means of lantern slides. The object is to translate a measurement from an external to an internal surface without error due to backlash or flexibility in the parts of the measuring machine. Referring to Fig. 7, the machine is seen to consist of a base carrying a right-angle bracket, which can be raised or lowered by means of a small bell-crank lever. On the top of the bracket an indicating lever is attached by a vertical crossed-spring pivot. The front end of this lever carries a double-pointed stylus, which makes contact with the surface of the gauge, while the movement of its rear end is magnified by an optical projection arrangement similar to that used in the small "millionth" measuring machine. The ring to be measured is attached to the vertical face of a carriage, which runs on balls in

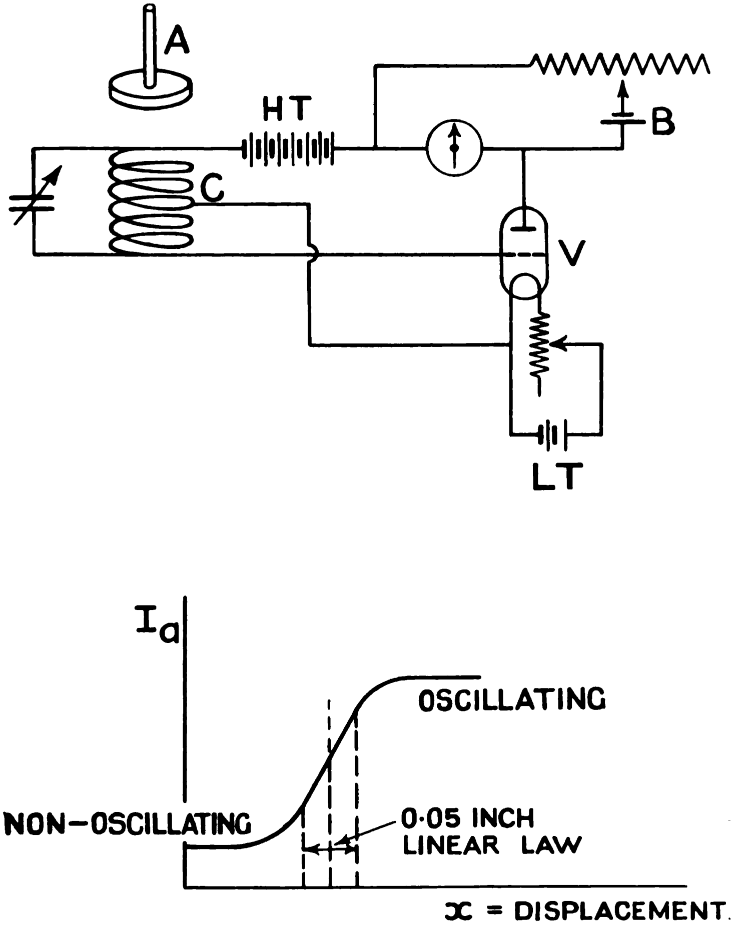


Fig. 6.

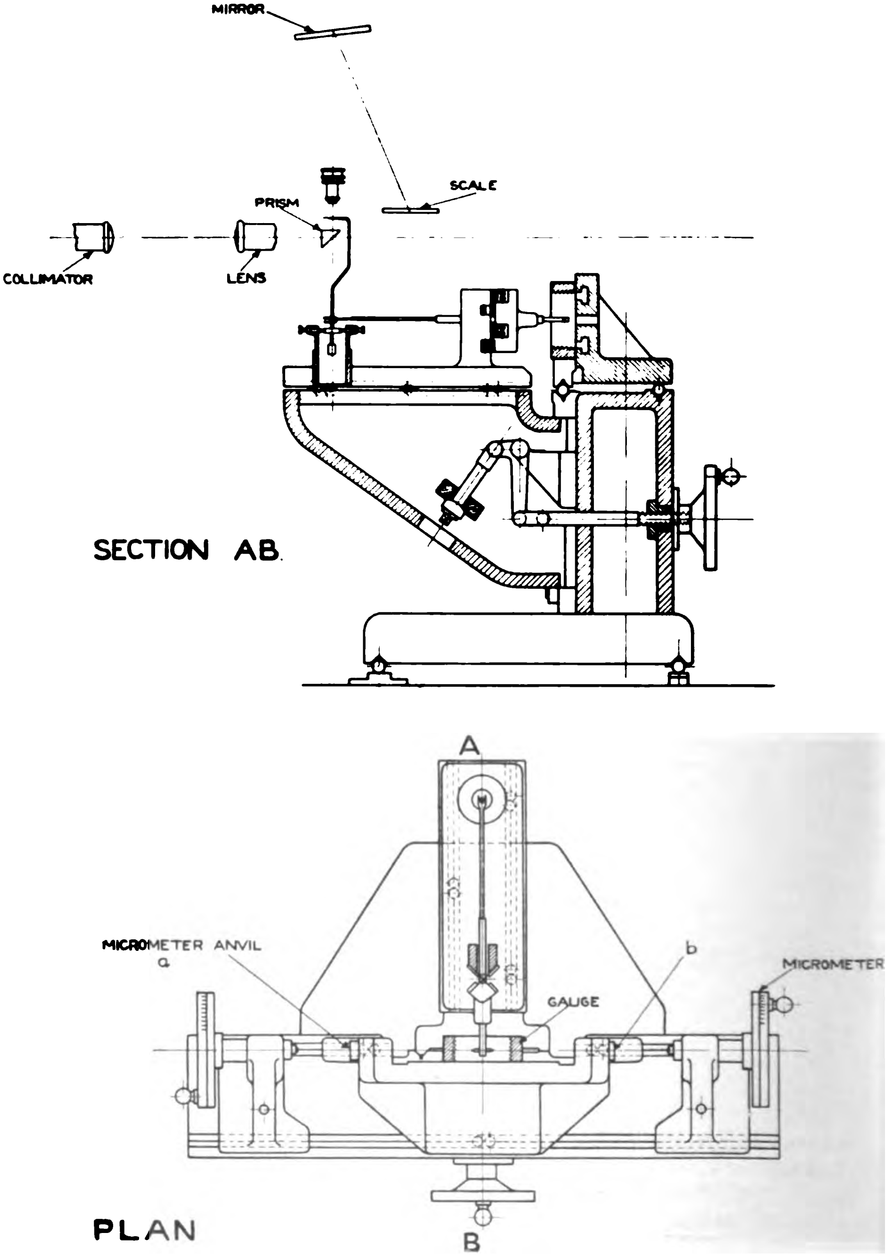


FIG. 7.—INTERNAL DIAMETER MEASURING MACHINE.

horizontal V-grooves, in the direction of the diameter of the ring. The movement of this carriage is controlled by two micrometers. When the stylus is in contact with one side of the ring, the bracket is raised or lowered until a maximum displacement of the indicating lever is observed, this serving to ensure that the contact is made on the diameter and not on a shorter chord. The corresponding micrometer is then adjusted till the indicator reaches a fixed fiducial mark, and its reading is taken. The same is done at the other end of the diameter, reading the second micrometer, and then a standard plug of known size is substituted for the ring, and the operations are repeated. Fig. 8 shows the procedure diagrammatically, and from this it is obvious that if x be the difference between the two readings taken on the first micrometer, and y the difference between those taken on the second, then the equation

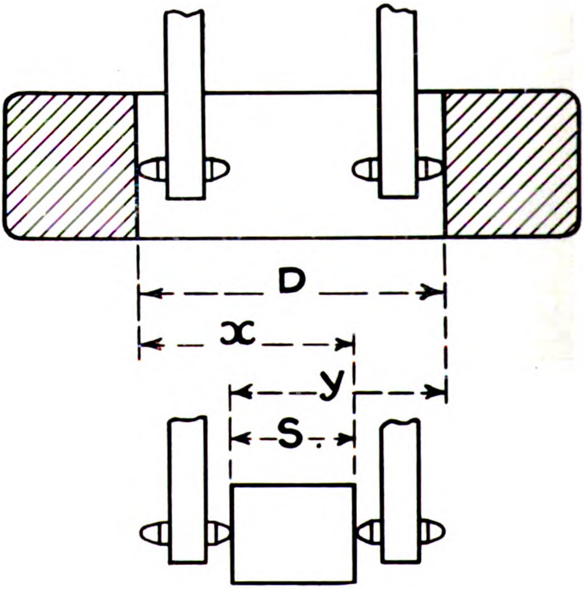
$$D = x + y - S$$

gives the diameter of the ring D in terms of the known plug S.

The important thing to notice is that both x and y are differences of two readings taken under identical mechanical conditions of the machine, so that any effect due to backlash or flexure of the indicating mechanism, in the process of transferring from external to internal measurement, has been eliminated.

The precise measurements of internal dimensions, which the introduction of these new machines has enabled us to make, have given us interesting and tolerably definite information on a subject which was formerly somewhat obscure—namely, the nature of the "fit" between a plain cylindrical plug and ring of nominally equal dimensions. I have on the table a ring which has been very accurately finished, and measured in the way I have described. Its diameter is approximately one inch. I have also a series of well-finished plugs, one of which is exactly equal in diameter to the ring. If I take these together, quite clean and dry, you will see that it is possible to insert the plug in the ring. It is a good fit, without any shake. If I take a plug 0·0001 inch smaller than the ring and assemble them dry, you will see that the fit is now quite easy. The ring will spin on the plug, or will fall along it under its own weight. But if I grease this same plug and then re-assemble the pair, the fit again becomes quite stiff. On the other hand, if I take a plug 0·0001 inch larger than the ring, while I cannot assemble them together at all in the dry condition, if I grease the plug well I have no great difficulty in doing so, and the ring is not an uncomfortably tight fit upon the plug. The effect of using grease, in fact, is to mask all minor differences in diameter which the experiments otherwise suffice to reveal. It is easy to show that in the last experiment the ring is actually expanded elastically when the plug is entered. I put the pair together on to the small measuring machine which I showed you earlier in the evening. Please note the indication on the scale. I now remove the plug from the ring and re-measure the latter along the same diameter. The indication has changed by three divisions, showing that the outside diameter of the ring was enlarged by 0·00006 inches when the greased plug 0·0001 inch oversize was inserted.

It must be understood, of course, that the success of these experiments depends on extremely accurate finish of both plugs and rings. Any slight irregularities of finish or deviation from circular or parallel form would be sufficient to falsify the results.



$D = (x + y) - S$

FIG. 8.—INTERNAL MEASUREMENT—DISPLACEMENT METHOD.

The last development to which I wish to call your attention this evening is one connected with the method of optical projection. During the war it became necessary to verify the accuracy of the profiles of numbers of gauges, in particular driving-band gauges for shell, and screw gauges, on a scale for which the comparatively slow methods previously available were entirely unsuited. Experiments were accordingly made by Mr. E. M. Eden at the N.P.L. with the object of measuring such objects by optical projection. After a considerable number of trials Mr. Eden was successful in finding certain combinations of lenses which gave an effectively undistorted projection on a scale of 50 to 1 over the whole of a field up to $1\frac{3}{4}$ inches diameter, i.e. about 7 feet diameter at the screen. The enlarged image of any profile could then be directly compared with a suitable large scale drawing, and the errors read off by means of an ordinary scale to an accuracy corresponding to about 0·0001 inch on the original. The advantage of such a method, where large numbers of complicated profiles have to be measured, will easily be appreciated. A more elaborate projection machine, specially intended for dealing with screw gauges, has also given invaluable service.

I want to call your attention particularly, however, to a still more recent development in the use of the optical projector. My colleague

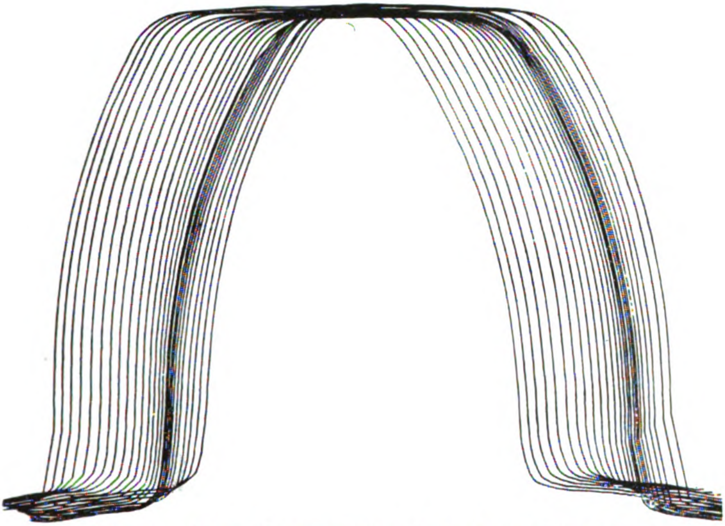


Fig. 9.—Gear Measurement—Pantograph Record.

Mr. Tomlinson has recently designed two machines for the purpose of verifying the accuracy of dimensions of gear wheels and gear-cutting hobs. It is not possible to examine such objects directly by the projection method, and Mr. Tomlinson therefore designed a special form of pantograph, which I have on the table before me, for obtaining records on smoked glass of the exact shapes of the teeth on the gears. The pantograph itself is a particularly interesting piece of work. Backlash has been entirely eliminated by a system of sprung pivots, and the accuracy of construction is such that, by the aid of certain necessary adjustments which are provided, it is possible to ensure an exact one-to-one reproduction of the original profile to within 0·0001 inch. It is necessary, of course, in comparing the record with a drawing to make allowance for the diameter of the ball point on the feeler arm of the pantograph.

I do not propose to describe in detail the machines with which this apparatus is used, but in explanation of the diagram, Fig. 9, which I now show you in illustration of the method, it should be mentioned that arrangements are provided by which the gear or hob can be rotated successively through a series of exactly equal angles. If these angles are made slightly less or slightly more than the nominal angular pitch of the teeth, a group record of all the teeth can be traced on a single slide, and when this record is projected on the screen, not only can the actual shape of the teeth be compared with a theoretically correct diagram, but the errors of spacing of the teeth can be directly seen and measured by observing the equality or otherwise of the spacing of the successive traces in the record.

While certain errors of comparatively large magnitude may be present in toothed gears without seriously affecting their performance, other errors, even if only very small, may lead to considerable noise and wear. The importance of this method of measurement in the case, for example, of gears for motor cars, or marine turbine reduction gears, will therefore readily be appreciated.

[J. E. S.]

WEEKLY EVENING MEETING,

Friday, May 28, 1926.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.,
Vice-President, in the Chair.

Sir Almroth Wright, K.B.E. C.B. M.D. F.R.S. M.R.I.

Aims and Methods of Therapeutic Research.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, June 4, 1926.

Sir Alfred Yarrow, Bart., LL.D. F.R.S., Vice-President,
in the Chair.

John Garstang, D.Sc. F.S.A.,
John Rankin Professor of Archæology, University of
Liverpool.

Researches in Palestine.

[Abstract.]

The organisation of research in Palestine and the consequent progress of excavation make it possible to fill in the established outline of the country's history with material detail at several points previously not illustrated, as may be seen from the following schedule of post-war investigations and results : –

PERIOD.		
Mediæval.		Exploration by the Metropolitan Museum of New York at Montfort.
		Exhibition of Crusaders' Remains and Mediæval Blazons at the Palestine Museum.
		Conservation Works by Government of Trans-Jordan at Kerak.
Roman		Excavations by Palestine Exploration Fund at Askalan.
		Conservation Works by the Government of Trans-Jordan at Jerash.
Hellenistic		Excavations by British School of Archæology at Tanturah (Dora).
E. Iron Age . . .	2.	Soundings by British School of Archæology at Tell Amr.
B.C. 1200	1.	Excavations by the Palestine Exploration Fund at Jerusalem.
		Excavations by British School of Archæology at Tell Harbaj.
		Excavations by American School of Archæology at Tell Mersim.
Bronze Age .	*Late*.	Excavations by University Museum of Philadelphia at Beisan.
		Excavations by University of Chicago at Megiddo.
	Middle.	Excavations by Palestine Exploration Fund at Askalan.
B.C. 2500 . .	*Early*.	Excavations by University of California at Tell Nasbeh.
		Excavations by British School of Archæology at Tell el Kassees.
Neolithic		

Palæolithic .	*Late.*	Excavation by the British School of Archæology of a Cave near Tabgha, containing flint implements and other prehistoric objects of Aurignacian type.
	Middle.	Excavation by the British School of Archæology of a Cave near Tabgha, containing Mousterian type flint implements and fossilised human remains of the Neanderthal type, together with other prehistoric fauna.
	Early.	Discovery by Officials of a field of Acheulian type flint implements near Abl in the Huleh Basin.

At Tell Hum (Capernaum) the partial rebuilding of the fallen synagogue has been undertaken by the Franciscan Custody; and the neighbouring ruins of a similar building at Kerazi (Chorazin) have been uncovered by the Department of Antiquities. Ten expeditions are at work excavating sites in Palestine this year.

The Lecturer illustrated more particularly the discovery of fossil remains at Tabgha, of Egyptian inscriptions and other materials at Megiddo and Beisan; and the uncovering at Jerash of the stage of the southern theatre, the main street and Nymphæum, the great gate and stairway of the northern temple, as well as Christian remains and the head of a statue.

GENERAL MONTHLY MEETING,

Monday, June 7, 1926.

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S. F.R.C.S.,
Treasurer and Vice-President, in the Chair.

The Chairman announced that, in terms of the Bye-Laws, Chap. V., Art. 2, the President, His Grace the Duke of Northumberland, had nominated the following Managers as Vice-Presidents for the ensuing year :—

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.
John Mitchell Bruce, C.V.O. M.D. LL.D. F.R.C.P.
Sir Ernest Moon, K.C.B. K.C. LL.B.
Sir Edward Pollock, F.R.C.S.
Sir Almroth Wright, K.B.E. C.B. D.Sc. M.D. F.R.S.
Sir Alfred Yarrow, Bart., F.R.S., M.Inst.C.E.
Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S. (Treasurer)
Sir Robert Robertson, K.B.E. D.Sc. F.R.S. (Secretary)

William Henry Inskip,
Mrs. James Mitchell,
Boris Orelkin,
Alexander Hamilton Rice, M.D.,
James Fowler Tocher, D.Sc. F.I.C.,

were elected Members.

The following Presents received since the last Meeting were laid on the table, and the Thanks of the Members were returned for the same :—

FROM

Machado, Prof. Dr. Virgilio (the Author)—Tempos Gloriosos. 8vo. 1917.

Parker, Dr. W. Rushton, M.D. M.R.I.—A Classical Dictionary of Greek and Roman Biography. By Sir Wm. Smith. Revised by G. E. Marindin. 4th edition. 8vo. 1925.

Concise Dictionary of Greek and Roman Antiquities. By Sir W. Smith. Edited by F. Warre Cornish. 8vo. 1898.

The Royal Mummies. By G. Elliot Smith. 4to. 1912.

Excavations at Kish. By S. Langdon. Vol. I., 1923-24. 8vo. 1925.

A Century of Excavation in Palestine. By R. A. S. Macalister. 8vo. 1925.

History of Greek Sculpture. By A. S. Murray. Revised edition. 2 vols. 8vo. 1890.

The Excavations at Babylon. By R. Koldewey. 8vo. 1914.

Murray's Classical Maps. Edited by G. P. Grundy:—
Asia Minor, Britannia, Eastern Empires, Gallia, Germania, Græcia, Hispana, Italia, Mare Ægæum, Palestine.

Wilson, J. S., Esq., M.R.I.—Report of Conference on Preservation of Waterloo Bridge. 4to. 1926.

WEEKLY EVENING MEETING,

Friday, June 11, 1926.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

J. C. M'LENNAN, LL.D. D.Sc. F.R.S. M.R.I.,
Professor of Physics and Director of the Physical Laboratory,
University of Toronto.

The Spectrum of the Aurora and the Constitution of the Upper Atmosphere.

A STUDY of the spectrum of the aurora has a direct bearing on the problem of the constitution and state of the upper atmosphere.

Our present knowledge of the upper atmosphere, apart from that gained by mountain climbing and by aeroplane ascents, or by the use of small free sounding balloons carrying automatically recording meteorographs, has been obtained hitherto chiefly from deductions made from observations on the flight and luminosity of meteors, from investigations on the abnormal audibility of sound waves generated by powerful explosions, and from investigations on the propagation and reflection of wireless ether waves. In addition, direct evidence of the presence of certain constituents in the upper atmosphere, such as ozone, water and carbon dioxide, for example, has been obtained from a study of the absorption spectrum of the sun's radiation.

1. *The Stratosphere.*—Aeronauts have flown as high as 13 km., i.e. approximately eight miles, or two and a half miles higher than the top of Mount Everest, and free sounding balloons carrying automatic instruments have reached heights a little beyond 25 km. Through these agencies we have learned that the temperature, as well as the pressure of the atmosphere, steadily decreases with elevation until a height of approximately 10 km. is reached. At this height a region is encountered known as the stratosphere, where the temperature is stationary at approximately 220° Abs., or 53° C. below zero. As to the height of the upper boundary of this stratosphere, no

information has yet been furnished by the records obtained with sounding balloons.

In the case of a sounding balloon recently released at Oxford, a height of 20·1 km. was attained before bursting took place, and the recording meteorograph, that was fortunately recovered, showed that the stratosphere was reached at 9·9 km. with a temperature of 231° Abs. Above this point not more than 2° change occurred at any point up to the maximum height of the balloon's flight.

2. *Lindemann and Dobson's Theory.*—From evidence provided by many observations on the flight and luminosity of meteors, Lindemann and Dobson have been led to conclude that the stratosphere does not extend beyond 55 km. or 60 km., and that at this height there is a rapid rise in the temperature of the atmosphere to approximately 300° Abs. The region in which the higher temperature is supposed to be maintained is the one in which the meteors are luminous, and since luminosity generally begins with meteors at a height of 150 km., this elevation is considered to represent approximately the upper boundary of this region of higher temperature.

3. *Audibility of Explosions.*—Evidence in support of the existence of this region of higher temperature in the upper atmosphere has recently been brought forward by F. J. W. Whipple in dealing with the abnormal audibility of explosions. In such a region the velocity of sound would be high, and Whipple has pointed out that this would account for the recurving of sound rays at great heights. This in turn enables one to offer an explanation of why explosions are often audible at some distance from the seat of the explosion while they are inaudible at intervening points. Such an explanation of the abnormal audibility of explosions demands, however, according to Whipple, that the transition from the stratosphere should take place at a height of approximately 30 km., and that in the superincumbent atmosphere a temperature even higher than 300° Abs. should exist.

4. *Propagation of Wireless Ether Waves.*—From experiments on the transmission of wireless ether waves it is now conceded that anomalies observed in the propagation of such waves can be explained by assuming the existence in the upper atmosphere of the so-called Kennelly-Heaviside conducting layer. The thickness of this layer of ionised gas has not as yet been determined, but it is believed to extend down to 40 or 50 km. from the ground by day, and to a height of about 90 km. by night, the rise in the under surface being due to the progressive recombination of ions at the lower levels after sunset. Assuming the conductivity of the layer to be due to the presence of electrons and positive molecular ions, Professor Chapman* has shown that the electron density in the conducting layer at about 90 km. is at least 100,000 per c.c. at night. In this connection Professor Chapman has also pointed out that an ionisation of this

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* Chapman, Q. J. Roy. Meteor. Soc., vol. 52, 1926, p. 225.

amount is ample to provide for the diurnal variations that occur in the earth's magnetic field without assuming an excessive thickness for the Kennelly-Heaviside layer.

5. *Ozone in the Upper Atmosphere.*—While the ionisation of the upper atmosphere by day might be attributed to the direct action of the ultra-violet light in the solar radiation, its persistence at night rather points to the formation by the sun's rays of some substance that gradually breaks up or dissociates into ions. The existence of such a delayed dissociation would account for the persistence of the ionised layer during the night, and for the rise observed in the height of its lower boundary on the approach of darkness. The evidence points to ozone as the substance able to fulfil these conditions. From the experiments of Fabry and Buisson,* as well as from those of Harrison and Dobson,† on the absorption of solar radiation, it is known that ozone exists in the upper atmosphere to such an extent that it is equivalent to a layer 3 mm. in thickness at normal pressure in temperature. It is probable that this ozone is formed out of oxygen by solar radiation of wave length shorter than λ 2000 Å. As ozone is known to absorb selectively radiation of wave length about $9 \cdot 5\ \mu$, as well as radiations in the visible and in the neighbourhood of λ 3000 Å, one would expect the region in the upper atmosphere where ozone predominates to be the one characterised by an abnormally high temperature, as well as one possessing high electrical conductivity. The evidence adduced above, it will be seen, seems to point to the approximate identity of the region in the upper atmosphere in which meteors exhibit luminosity with the one that is richest in ozone, and incidentally with the one responsible for the reflection of wireless waves. As 150 km. would seem to be the height of the upper boundary of this region, it follows, if we assume that ozone is present in the atmosphere at this height, that oxygen also must be present. Moreover, on account of its higher density, ozone when found must gradually sink down, and consequently one may reasonably conclude that oxygen exists in the atmosphere at heights very much greater than 150 km.

6. *The Aurora.*—Of all meteorological phenomena auroral displays are considered by many among the most interesting. They occur in both hemispheres, that in the northern being designated as *Aurora Borealis* and that in the south as *Aurora Australis.* The frequency of their occurrence varies with latitude, and is greatest in the two zonal regions that make angles of about 20° with the earth's magnetic axis.

The aurora appears with varying intensities, and in a variety of forms and colours. At times it is seen as a weak diffuse formless luminosity. At other times it appears as luminous draperies, bands or arcs, and then again as luminous beams or rays. These forms are som[illegible]onary, and at other times undergo movements more or less rapid. Even pulsating types of luminosity have been observed. The feebly luminous diffuse types of aurora are usually yellowish-green or greenish-white ; but blue, red, violet and other colours may occur in the more intense and moving varieties. In the case of curtain or drapery-like forms the upper portion has frequently a greenish colour, while the lower edge is of a deep reddish or purplish hue.

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* Fabry and Buisson, Astrophysical Jnl., vol. 54, 1921, p. 297.

† Harrison and Dobson, Q. J. Roy. Meteor. Soc., vol. 51, 1925, p. 363.

Since 1907 Professor Störmer has devoted much time and effort in Norway at Oslo, Bygdø, Bossekop, and other stations towards the determination of the height and position of auroral displays. The method used involved the simultaneous photography of an aurora from two or more stations 4·5 km. to 258 km. apart. With the instruments that he has devised and the technique developed he has found it possible to obtain good photographs with but one second exposure. A pair of such photographs, most kindly supplied by him for this lecture, is shown in Figs. 1 and 2. They were taken at the time of the great auroral display on March last. Fig. 1 shows an auroral curtain photographed by Professor Störmer at Bygdø at 18 h. 36 min. on the day in question. The stars γ, δ, ϵ, ζ and η of the Great Bear are shown very clearly in the picture. Fig. 2 shows the same auroral curtain photographed simultaneously by Carsten Borchgrevink at Tømte, 48 km. to the N.N.W. of Bygdø. The parallax referred to the same stars, it will be seen, is very great. Professor Störmer states that measurements made on these plates and calculations based on them showed that the altitude of the lower border of the auroral curtain was unusually high ; four points at the foot of the rays gave the altitudes 135, 132, 131 and 137 km. above the earth. The summits of their rays reached at least a height of 350 km.

From hundreds of photographs taken by Professor Störmer he has calculated the heights of the upper and lower edges of the auroral displays, and these plotted for Bgydø and Bossekop are shown in Figs. 3 and 4. From these plottings it will be seen that very few displays occur below 80 km., but above this level the number increases rapidly, the most frequent occurrences being those beginning at a height of 106 km. Above 106 km. the number of auroræ fall off rapidly. Some have been observed to extend as high as, or even higher than, 500 km., and the greatest height at which any recorded auroral display has been observed is 750 km. A point of some interest that has emerged from Professor Störmer's work is that the auroral displays as observed at Oslo extend much higher in the atmosphere than when observed at Bossekop near Finland.

7. *The Auroral Spectrum.*—The earliest observation of the spectrum of the aurora appears to have been made by Ångström* in

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* Angström, Pogg. Ann., vol. 137, 1869, pp. 161-163.

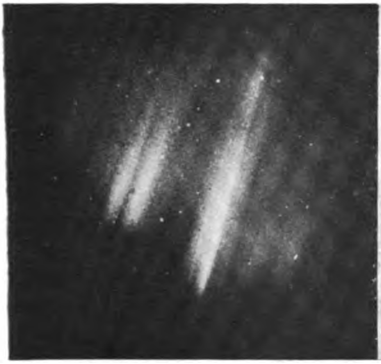

Fig. 1.—Auroral curtain photographed at Bygdø at 18h. 36m. The stars γ, δ, ϵ, ζ and η of the Great Bear are seen very well in the picture.

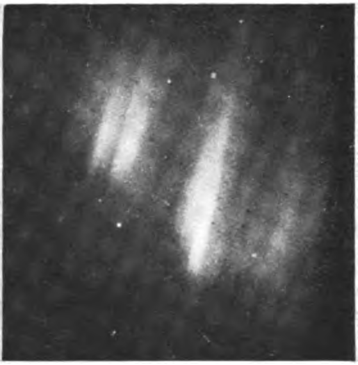

Fig. 2.—The same curtain photographed simultaneously by Carsten Borchgrevink at Tømte.

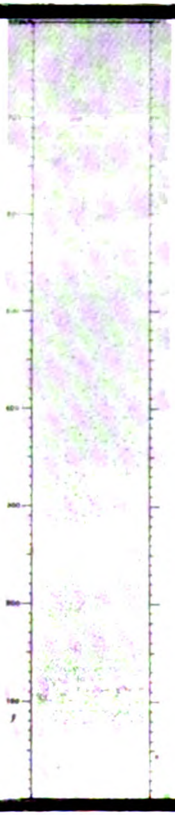

FIG. 3.—Plotted from observations at Bygdø.

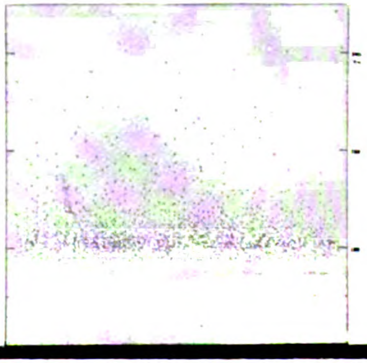

FIG. 4.—Plotted from observations at Bossekop.

1869. He expressed the opinion that one should expect the spectrum to be the same as that of the electrical discharge in air, but it turned out otherwise, for he found only a "green line" at λ 5567 Å, and in addition three very faint lines in the blue region, all of which appeared to be of unknown origin.

Since the time of Ångström a number of investigators have studied the auroral spectrum, but with the exception of Gyllenskiold's work in 1886 nothing of an extensive nature appears to have been done until the brilliant series of studies undertaken by Professor Vegard* was begun in the winter of 1912–13. The results of Vegard's work, as collated and summarised by him, are given in Table I. In addition a spectral line or band of weak intensity has been observed in the spectrum of the aurora, whose wave length has been variously given by different observers as λ 5269 Å, λ 5205 Å, λ 5200 Å, λ 5233 Å, λ 5210 Å, λ 5239 Å, λ 5207 Å, λ 520 B, λ 5228 Å, λ 5235 Å, λ 5166 Å, λ 5230 Å.

An examination of the wave lengths given in the table reveals a number of interesting points. In the first place, the list contains no wave lengths that correspond to any radiation emitted by either hydrogen or helium. This would seem to indicate that neither hydrogen nor helium are present in the upper atmosphere to any appreciable extent. Again, the wave lengths λ 4708·7 Å, λ 4651·1 Å, λ 4728 Å and λ 3914 Å are of special interest, for they represent one of the outstanding features of the auroral spectrum. Fig. 5 contains a number of reproductions of photographs of the auroral spectrum taken by Professor Vegard. They are designated by the letters N.L. (Northern Lights). A glance at these photographs shows at once that the bands λ 4708·7 Å, λ 4651·1 Å, λ 4278 Å and λ 3914 Å are among the most striking characteristics of the spectrum. Fig. 6 is also a spectrogram of the auroral light. It was taken by Professor Slipher during an auroral display in May 1921, and kindly sent to me by him for use in this lecture. The bands referred to are shown very prominently on it. Again, the same feature is well brought out in a photograph of the auroral spectrum taken by Lord Rayleigh in 1921, and reproduced in Fig. 7. This photograph by Lord Rayleigh possesses, however, additional and very special interest, for it shows a spectrum of nitrogen contiguous to that of the aurora, both spectra having been taken with the same instrument. In the reproduction of the nitrogen spectrum the bands shown are the well-known negative bands of nitrogen, λ 4708·7 Å, λ 4278 Å and λ 3914 Å. Their identity with the bands of the auroral spectrum is unmistakable, and it is now universally conceded that these par-

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* Vegard, Phil. Mag., 6 S. Vol. 46, 1923, pp. 193, 577; Ann. d. Phys., Bd. 79, 1926, p. 377.

TABLE I.

Auroral lines observed with spectrograph			Lines of nitrogen spectrum		
I.	II.	III.	—	Type	Observer
Å	Å	Å	Å		
3165·0	—	—	3135·9	P.B.	Hermesdorf
3160·0	—	—	3159·2	"	"
3208·3	—	—	?	"	"
3284·9	—	—	3285·3	"	"
3371·3	—	—	3371·5	"	"
3432·7	—	—	?	"	"
3467·8	—	—	3468·1	"	Deslandres
3502·9	--	—	3500·5	"	Hermesdorf
3536·1	—	—	3536·8	"	"
3576·9	—	—	3577·0	"	"
3711·1	—	—	3710·7	"	"
3755·7	—	3758·5	3755·5	"	"
3805·4	—	3807·0	3805·1	"	"
3918·8	3913·3	3913·7	3914·4	N.B.	Deslandres
—	—	3941·5	3943·1	P.B.	Hermesdorf
3998·0	—	4000·4	3998·5	"	"
4056·2	—	4059·7	4058·7	"	Hasselberg
—	—	4182·5	?		
—	—	4200·0	4210·0	"	Hasselberg Or N.B. 4198·8
4237·8	4236·3	4238·4	4236·3	"	Hasselberg
—	4266·8	—	4269·4	P.B.	Hasselberg Diffuse line, perhaps shade of a band
4277·7	4277·9	4279·0	4278·0	N.B.	Hasselberg
—	—	4345·8	4343·8	P.B.	"
—	—	4378·9	4379·8	L.S.	Hemsalech
—	—	4426·5	4426·2	"	"
4421·5	?	4478 5	4478·0	"	"
—	—	4552·1	4552·3	"	"
—	—	4591·9	4590·0	"	Neovius
4652·7	4651·1	4650·8	4651·2	N.B.	Hasselberg
4707·7	4708·7	4708·3	4708·6	"	"
—	—	4779·2	4779·0	L.S.	Thalen
—	—	4857·4	4860·6	"	Hemsalech
—	5578·2	5578·4	?	"	
		5925 6465 } bands	Probably pos. bands of N		

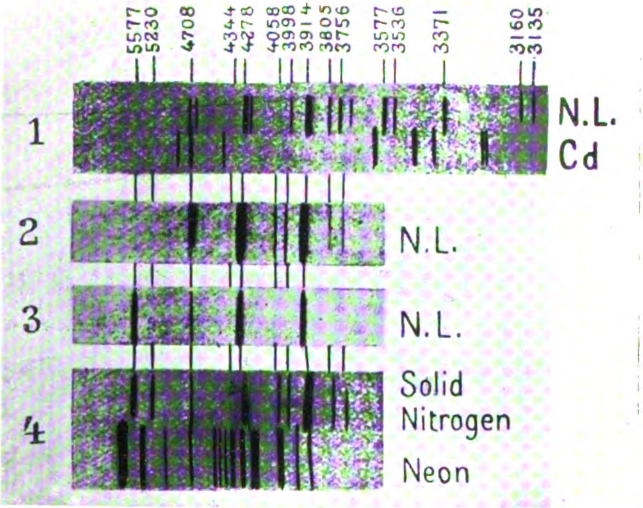

FIG. 5.—The Spectrum of the Northern Lights by Prof. Vegard.

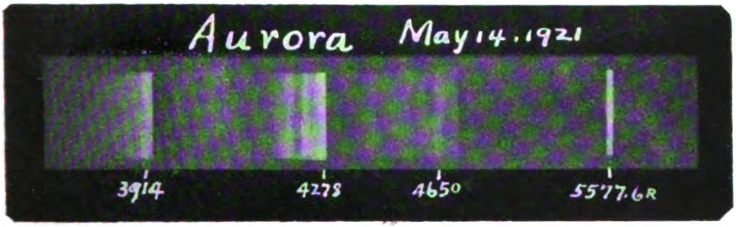

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Fig. 6.—The Spectrum of an Aurora by Dr. Slipher.

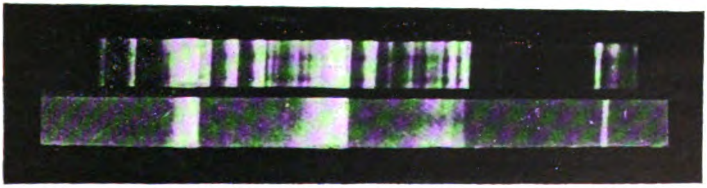

FIG. 7.—The Spectrum of an Aurora and of the Negative Bands of Nitrogen by Lord Rayleigh.

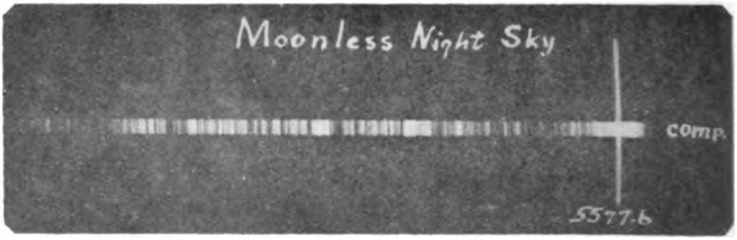

FIG. 8.—The Spectrum of the Light from the Moonless Night Sky by Dr. Slipher.

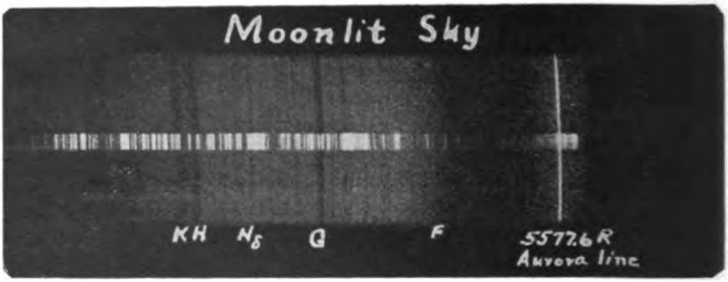

FIG. 9.—The Spectrum of the Light from the Moonlit Sky by Dr. Slipher.

ticular bands, which constitute such a prominent feature of the auroral spectrum, clearly establish the presence of nitrogen in the upper atmosphere in the same form and in the same state as we have it at the surface of the earth and in our laboratories.

But the most outstanding feature by far of the auroral spectrum is the radiation of wave length λ 5577 Å, that appears in the auroral spectrum as a sharp, strong, narrow, well-defined spectral line. It is well brought out in the reproduction of the auroral spectra shown in Figs. 5, 6 and 7. This spectral line long remained a puzzle, and until recently all attempts to identify its origin ended in failure.

In some observations made by Professor L. Vegard it was found practicable to photograph the spectrum of the aurora with the light emitted from parts of an auroral display, these parts being at various heights. Photographs so taken showed that the radiation corresponding to the green line, as well as that corresponding to the nitrogen bands mentioned previously, was emitted during a display up to the extreme upper limit of auroral rays, reaching altitudes of several hundred kilometres. Moreover, it appeared from the spectrograms that the intensity of the green line as compared with that of the negative nitrogen bands varied but little over the whole area of the display. If there was any change in the relative intensity at all it was very small, and went in such a direction as to make the negative nitrogen bands relatively stronger with increase in height of the point of origin of the light examined.

8. *Spectrum of the Light of the Night Sky.*—Added interest became attached to this spectral line when it was shown by Dr. W. W. Campbell, Lord Rayleigh, Dr. Slipher, Babcock, and others, that a spectrum could be obtained from the light of the moonless, cloudless night sky whose characteristic and possibly only feature was this puzzling line. Photographs of this spectrum, also taken and kindly sent to me by Dr. Slipher, are shown in Figs. 8 and 9, the one having been taken on a moonless night and the other on a moonlit one. This spectrum can be obtained from the cloudless night sky in all latitudes, whether any polar aurora is in evidence or not. Indeed it would seem that it can be obtained from any part of the sky in the lower latitudes with greater intensity than in latitudes nearer the poles.

9. *Wave Length of the Auroral Green Line.*—Two years ago Harold D. Babcock made a study of this line in the spectrum of the light of the night sky, with a Fabry-Perot interferometer, on Mount Wilson and at Pasadena. He showed that it could be photographed with but one night's exposure with this instrument on almost any night between the third and first quarters of the moon, even when the sky was partly clouded. No prism or other dispersion apparatus was required. The *étalon* was simply mounted in front of a high-speed camera supplied with very rapid green-sensitive plates, and provided with a colour screen when there was much moonlight or diffuse

artificial light. Orders of interference amounting to λ 3700, λ 8450, λ 15,500, λ 31,000 and λ 85,000 waves were successively used in photographing the interference rings, and from their sharpness it was concluded that the width of the line was not greater than 0·035 Å. A reproduction of one of Babcock's photographs of the interference pattern taken with the light of the auroral line is shown in Fig. 10.

In Fig. 11 one is also shown of the ring system of the mercury spectral line λ 5461 Å taken by him with the same instrument. A careful determination of the wave length of the line made by Babcock showed it to be λ 5577·35 IA, with an uncertainty of not more than λ 000·5 Å.

The work of Babcock is specially important, for it showed that in seeking for an origin for the auroral green line by laboratory experiments one had to look for a spectral line whose sharpness was of the same order as that of the lines in the spectrum of the iron arc, and whose wave length was known to within one-hundredth of an Ångström.

An additional point that I should note here in connection with all photographs of the spectrum of the light of the night sky taken hitherto, is that on none of them has there ever appeared any trace of the negative bands of nitrogen λ 4708·7 Å, λ 4278 Å, 3914 Å that are always such a prominent feature of the spectrum of the polar aurora. This would seem to indicate that one must look elsewhere than to nitrogen, at least in its ordinary state, for the origin of the famous green spectral line.

10. *Vegard's Solid Nitrogen Theory.*—In July, 1923, interest in the auroral spectrum was suddenly stimulated and intensified by a suggestion, put forward by Professor Vegard, that the auroral green line would be found to have its origin in solid nitrogen held in suspension in the upper atmosphere in a fine state of division. This remarkable suggestion was followed early in 1924 by an announcement by Vegard that solid nitrogen when bombarded by swiftly moving electrons became luminescent, and showed a line broad and possibly composite in its spectrum at λ 5577 Å; also one at λ 5231 Å. This result at once appeared to be a remarkably brilliant confirmation of Vegard's bold suggestion. Some time later, however, the present lecturer announced that in collaboration with his associate, Mr. G. M. Shrum, he too had been investigating the spectrum of solidified nitrogen rendered luminous by cathode rays, and that instead of a single spectral line at λ 5577 Å had found the luminescent spectrum to consist of three broad lines or narrow bands, with the mean wave lengths λ 5556 Å, λ 5617 Å and λ 5654 Å. It also transpired that the line at λ 5231 Å was due to solid nitrogen rendered *phosphorescent* by bombardment with high-speed electrons. The luminescence and phosphorescence spectra of solid nitrogen as

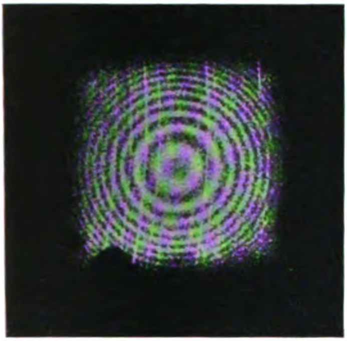

FIG. 10.—Interference rings formed by the light of the Auroral Green Line by Babcock.

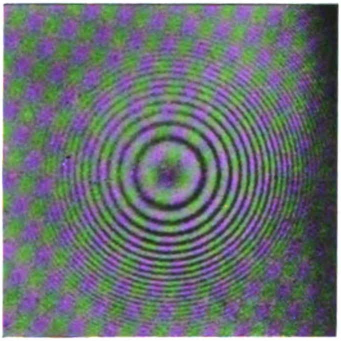

FIG. 11.—Interference rings formed by the light of the Mercury Green Line 5461 Å.

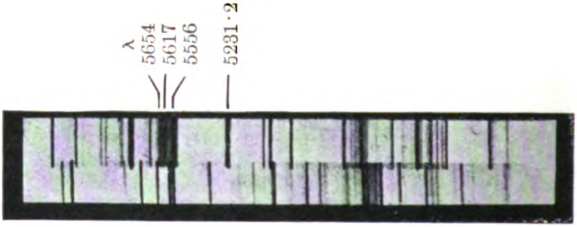

FIG. 12.—The wave lengths of the bands of the Luminescence and Phosphorescence Spectra of Solid Nitrogen.

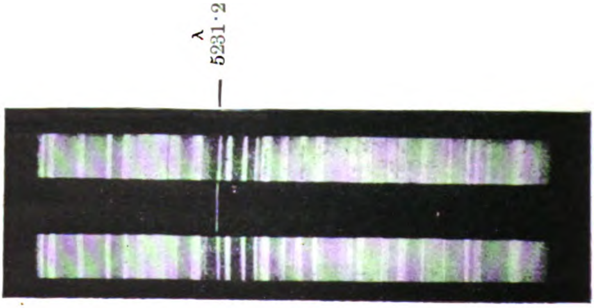

FIG. 13.—The Phosphorescence Spectrum of Solid Nitrogen.

originally observed in our experiments is shown in Figs. 12 and 13. The bands λ 5617 Å, λ 5654 Å and λ 5556 Å were obtained with the light from solid nitrogen while it was being bombarded by electrons, and the line or narrow band λ 5231 Å with the light from solid nitrogen when phosphorescing after the bombardment had ceased. This phosphorescence of nitrogen with its extremely simple spectrum is remarkable in that it appears to be the first instance recorded of phosphorescence being exhibited by an element in a state supposedly very pure.

In the interval that has elapsed since the discovery that solid nitrogen could be rendered luminous and phosphorescent, the subject of the luminescence of solid nitrogen has been very fully investigated by Professor Vegard in Europe, and by myself in association with my colleagues and students in Toronto.

In our later experiments, carried out with the help of my research workers H. J. C. Ireton and K. Thomson, we have succeeded in intensifying the effect by making use of tubes of a type recently designed by Dr. Coolidge, of Schenectady. These tubes were provided with windows of thin nickel foil through which electrons under a potential of 100,000 volts R.M.S. were projected into the vacuum space of a german-silver thermos flask of special design. The nitrogen was deposited and solidified on the inner wall of the thermos flask within the vacuum space, and the refrigerant used was liquid hydrogen.

The luminescence of the solid nitrogen was observed in our experiments through a fluorite window in the outer wall of the thermos flask, and its spectrum was photographed with glass, quartz and fluorite spectrographs. All wave lengths measured and given below are in Ångström units.

Photographs of the spectrum of the light emitted while the solid nitrogen was being bombarded showed :—

(1) A single sharply defined narrow band (N_4) at λ 5945.

(2) Three broad diffuse bands (N_1) shading each into the other with mean wave lengths of approximately λ 5554, λ 5617, λ 5658.

(3) A series of eight clearly defined bands (N_2) with the wave lengths λ 5204·4, λ 5210·4, λ 5214·3, λ 5220·4, λ 5224·4, λ 5228·8, λ 5235, λ 5240.

(4) A number of faint diffuse bands, each shaded off towards the red in the blue and violet spectral region between λ 4500 and λ 2460. Very approximate wave lengths of the heads of these bands were found to be λ 4575, λ 4500, λ 4270, λ 3990, λ 3725, λ 3510, λ 3385, λ 3155, λ 2960, λ 2785, λ 2615, λ 2460. As regards these bands some have been observed by Vegard, and some appear to belong to the well-known second positive band spectrum of nitrogen.

Among a number of interesting results obtained by us, four are worthy of special note :—

(1) We failed to record on our plates, with twenty hours' exposure, any trace of a group of wave lengths designated by Vegard as N_3 that were found by him to include one band at λ 6569, and one with its maximum intensity between λ 6320 and λ 6420. In this connection it might be stated that our plates were highly sensitive up to λ 7000.

(2) When the tube was in action and the solid nitrogen was strongly luminescent, we always found by visual observation, that when the bombardment was interrupted, the wave lengths of the group N_1 immediately disappeared from the radiation emitted by the luminous nitrogen, while the red band N_4 and the bands N_2 between λ 5204·4 and λ 5240 persisted some minutes, often very brilliant, but with slowly weakening intensity. Vegard has suggested that groups N_2 and N_4 belong to the phosphorescence spectrum of solid nitrogen, and, as will be seen from the results given above, our experiments confirm this view.

(3) As to the group of three bands N_1, we found that they were relatively strong when the solid nitrogen under bombardment had been freshly deposited. But with prolonged bombardment of a given layer of solid nitrogen, the intensity of the group of wave lengths N_1 rapidly weakened, and finally was scarcely sufficient to make the radiation observable. The intensities of the band groups N_2 and N_4, on the other hand, appeared to be unaffected by prolonged bombardment. Since exposures of long duration were required in photographing the complete luminescence spectrum, it was necessary therefore to deposit a fresh layer of solid nitrogen approximately every half-hour.

From these results it is clear that two spectra are obtainable from solid nitrogen, one of which includes the band group N_1 and the other the bands N_2 and N_4. This would go to show that solid nitrogen can exist in two forms, A and B, the one, A, emitting the radiation constituting the band N_1, and the other, B, the radiation constituting the band groups N_2 and N_4. Our experiments support the view that the solid nitrogen is initially deposited in the form A, and that under electronic bombardment, and possibly otherwise, it is soon transmuted into the form B, this form B being the one that exhibits the phenomenon of phosphorescence.

(4) The band group N_2, as originally observed by Vegard and by M'Lennan and Shrum, consisted of a single sharply defined narrow band at approximately λ 5230. This band has been found, however, to be more extensive than was originally supposed, for, as shown above, it is known to consist of at least eight strong and well-defined members.

The bands N_1, N_2 and N_4 are shown in Fig. 14, the bands N_1 in Fig. 15, and the bands N_2 in Fig. 16.

In a lecture having the character of the present one it is impossible to go into details, and it must suffice to point out that the theory of Vegard, though brilliantly conceived, is not supported by the spectral evidence obtainable from the luminescence of solid

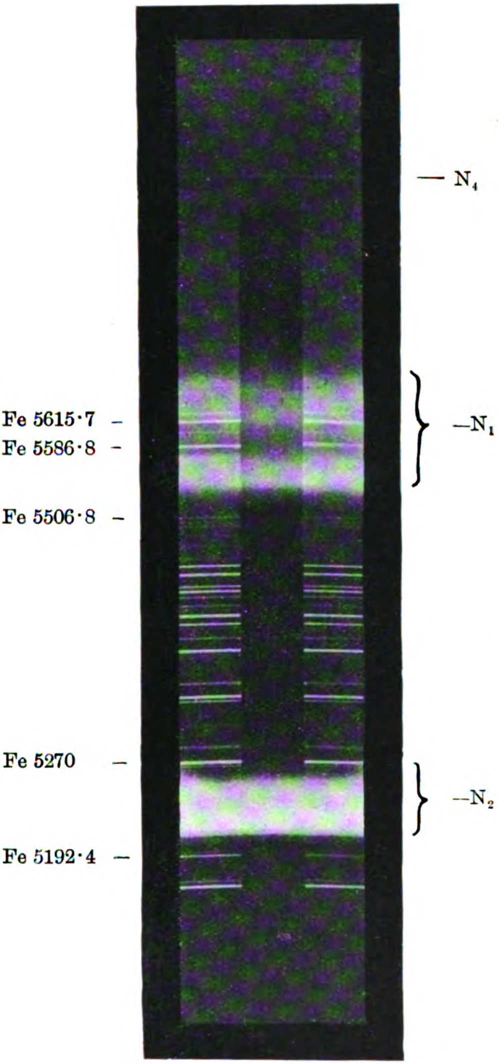

Fig. 14.—The bands N_1, N_2 and N_4 in the Spectrum of Solid Nitrogen.

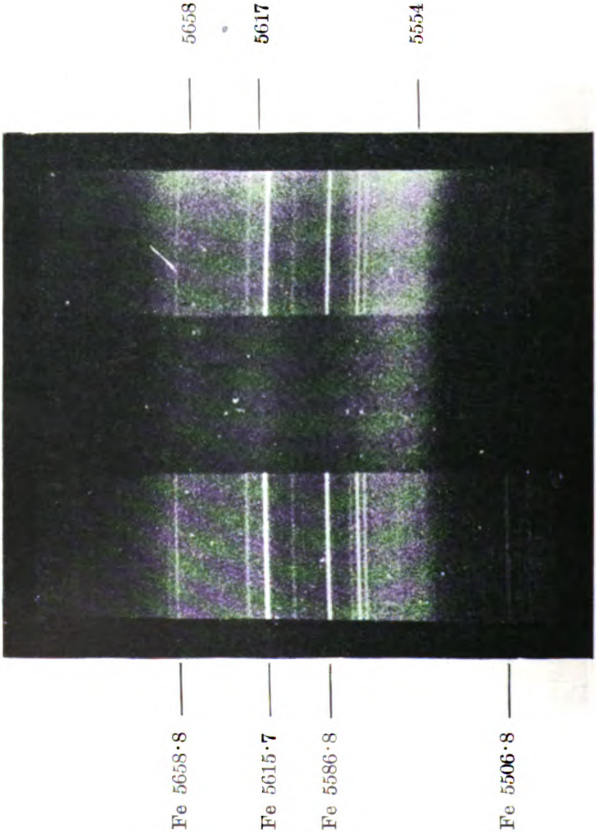

FIG. 15.—The bands N_1 of the Luminescence Spectrum of Solid Nitrogen.

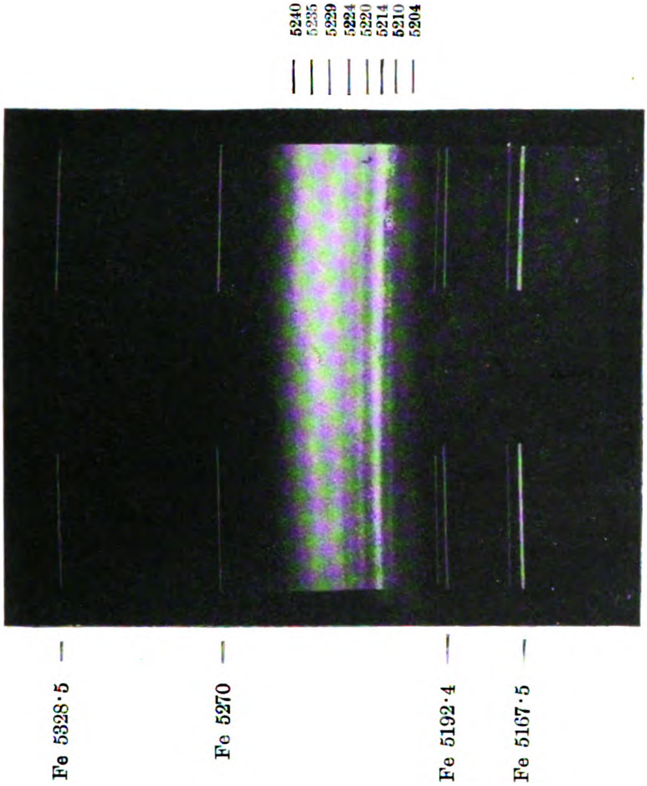

Fig. 16.—The eight bands N_2 in the Phosphorescence Spectrum of Solid Nitrogen.

nitrogen. It is quite clear from Fig. 15 that the group of bands we have designated as N_1, with wave lengths λ 5556 Å, λ 5617 Å, λ 5654 Å, are broad and diffuse, and have none of the characteristics of the wave length λ 5577·35 Å of the auroral spectrum. It is impossible to identify any one of these bands or any portion of any one of them with the auroral line. Moreover, if solid nitrogen in a state of fine division were diffused throughout the upper atmosphere it would certainly be rendered phosphorescent, and the auroral spectrum would contain a strong and well-marked composite band at or near λ 5230 Å having the characteristics of the group of eight bands N_2 shown in Fig. 16. Up to the present no band possessing these features has ever appeared in photographs of auroral spectra or in those of spectra of the light from the night sky. We are therefore forced to the conclusion that the auroral green line does not originate in solid nitrogen, and that there is no valid evidence that in the upper atmosphere, in the region in which the auroral light originates, the temperature ever reaches such a low value as that required to solidify nitrogen.

11. *Oxygen and the Auroral Green Line.*—The search for the origin of the auroral green line took on a new aspect a year ago when in collaboration with my assistant, Dr. G. M. Shrum, I was able to show that it was possible to obtain a spectral line from an electric discharge in a mixture of oxygen and one or other of the rare gases helium and neon, with the rare gas in excess, that had all the characteristics of the auroral green line. It was also obtained by us faintly in pure oxygen at low pressures. Measurements carefully made showed its wave length within the limits of error to be λ 5577·35 Å. As will be seen from the reproduction shown in Figs. 17 and 18, it can be obtained as a very sharp clearly defined line. Its intensity, depending as it does on a number of factors not yet clearly understood, is subject to great fluctuations. During the past few months I have made a careful study of the characteristics of this spectral line, in association with my research workers, Mr. J. H. McLeod and Professor W. C. McQuarrie, and have found that this spectral line λ 5577·35 Å can never be observed in the spectrum of any electrical discharge in the absence of oxygen. With pure oxygen we have found its intensity is strongest with the gas at a pressure of 2 mm. of mercury, when the exciting electrical discharge was passed through a tube about 1 m. long and about 3 cm. in diameter.

When currents varying in strength up to 160 milliamperes were used the intensity of the line steadily increased with the exciting current. When an electrical discharge was passed through oxygen mixed with helium, with the former at a pressure of 2 mm. of mercury, the line was obtained with strongest intensity when the partial pressure of the helium was between 15 and 20 mm. of mercury. In a series of carefully executed experiments we found that the power possessed by the rare gases of enhancing the line relative to an ordinary arc line of oxygen, such as λ 5437 Å, assuming this relative intensity in oxygen alone to be 1, was as follows: helium 1·7, neon 4·6, argon 84·2. In some of our experiments we found that when argon in excess was mixed with oxygen the line was obtained with an intensity greater than that of any known line in the spectrum of atomic oxygen having a wave length shorter than λ 6000 Å. Viewed under these circumstances with an echelon spectroscope that had a resolving power greater than 200,000, the line still appeared sharp. Moreover, it was single and unaccompanied by any satellites.

12. *Zeeman Effect with* λ 6577·35 *Å*.—Some preliminary experiments were made to obtain the Zeeman resolution of this oxygen line λ 5577·35 Å in a magnetic field, and for this purpose the discharge tube was placed along the axis of a coil capable of producing a magnetic field of 3,600 Gauss. When the line was viewed by means of the echelon spectroscope just mentioned, it appeared under longitudinal observation as a beautifully clear doublet. Visual measurements of the separation of this doublet showed it to be equal to that between the outer components of a normal triplet. When the discharge was viewed in a direction perpendicular to the magnetic field the line was too weak for its central component, which doubtless existed, to be seen. This type of magnetic resolution may be taken as evidence that the line λ 5577·35 Å is an atomic spectral line, and that it has its origin in an element whose atoms possess levels characterised by odd multiplicity. As a general rule the spectral lines of elements, whose arc spectral terms are of odd multiplicity, break up in a magnetic field into an odd number of components, with one of them in the position of the original line. On the other hand, the lines of elements with arc terms of even multiplicity generally break up into an even number of components, without any component occupying the position of the original line. Up to the present no one has ever in any spectrum observed a line that showed a simple doublet magnetic resolution, least of all one with a doublet separation equal to that between the outer components of a normal triplet. As the terms that constitute the basis of the arc spectrum of oxygen are of odd multiplicity, and as it is highly probable from our work on the Zeeman effect that the line λ 5577·35 Å is resolvable into a normal triplet, we have in the magnetic resolution of the line additional support for our view that the line originates in oxygen.

13. *Spectral Terms of Oxygen*.—Until recently it seemed impossible to provide a place for the wave length λ 5577·35 Å in the series and term scheme of the arc spectrum of oxygen. This scheme, as it was known up to a short time ago, is graphically represented in Fig. 19, and is based, it will be seen, on a series of terms representing

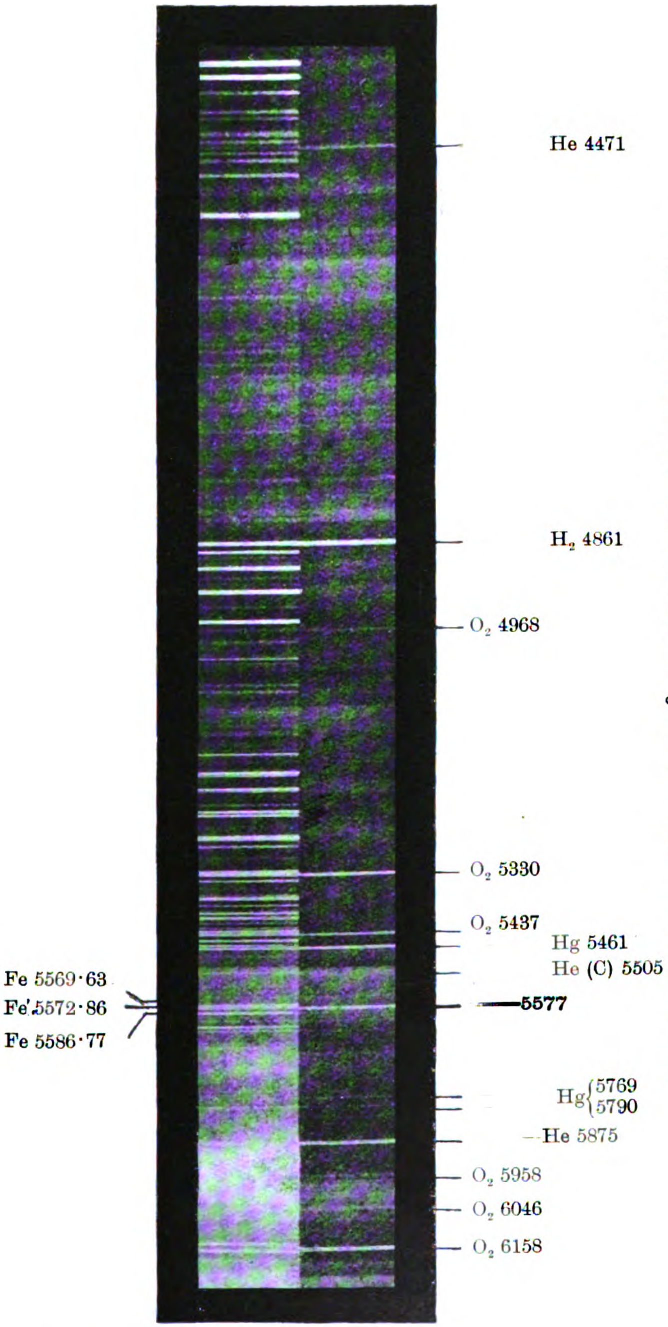

FIG. 17.—The Spectral Line = 5577·35 Å obtained with a mixture of Oxygen and Helium.

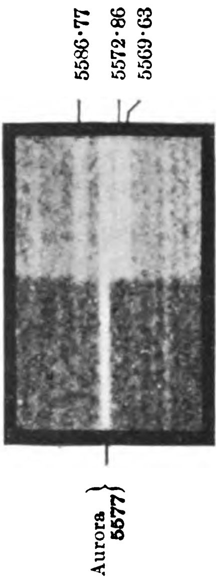

FIG. 18.—An enlargement of a portion of Fig. 17.

atomic levels of triple or quintuple multiplicity. These are given in Division A of Table II., and corresponding to them there is also given in the table the configurations of the extra nuclear electrons of the atoms of oxygen that give rise to them. Wave lengths in the arc spectrum of oxygen known hitherto are provided for in the scheme of Division A, and it will be readily seen there is no possibility of including the wave length λ 5577·35 Å in it. This does not exclude the possibility, however, of the line λ 5577·35 Å being an arc line in the atomic spectrum of oxygen, for the lecturer, in collaboration with his students, H. Grayson Smith and A. B. McLay, has been able to show recently that, following the theory dealing with spectral terms put forward by Heisenberg and Hund, two sets

TABLE II.—SPECTRAL TERMS.

Oxygen.

A		B	
N_K 1_1, 2_1 2_2, 3_1 3_2 3_3	Terms	N_K 1_1, 2_1 2_2, 3_1 3_2 3_3	Terms
2, 2 3, — — —,	$^4S_2$	2, 2 3, — — —,	$^2D_{23}$
2, 2 4, — — —,	$^3P_{012}$	2, 2 4, — — —,	$^1\bar{D}_2$
2, 2 3, 1 — —,	$^5S_2$ $^3S_1$	2, 2 3, 1 — —,	$^3D_{124}$ $^1D_2$
2, 2 3, — 1 —,	$^5P_{123}$ $^2P_{012}$	2, 2 3, — 1 —,	$^3F_{234}$ $^3\bar{D}_{123}$ $^3P_{012}$
2, 2 3, — — 1,	$^5D_{1234}$ $^3D_{123}$		$^1F_3$ $^1\bar{D}_2$ $^1P_1$
		2, 2 3, — — 1,	$^3G_{345}$ $^3\bar{F}_{234}$ $^3D_{123}$
			$^3\bar{P}_{012}$ $^3S_1$ $^1G_4$
			$^1\bar{F}_3$ $^1D_3$ $^1\bar{P}_1$ $^1S_0$

C	
N_K 1_1, 2_1 2_2, 3_1 3_2 3_3	Terms
2, 2 3, — — —,	$^2P_{12}$
2, 2 4, — — —,	$^1\bar{S}_0$
2, 2 3, 1 — —,	$^3\bar{P}_{012}$ $^1P_1$
2, 2 3, — 1 —,	$^3\bar{D}_{123}$ $^3P_{012}$ $^3\bar{S}_1$
	$^1\bar{D}_2$ $^1P_1$ $^1\bar{S}_0$
2, 2 3, — — 1,	^3F ^3D ^3P
	^1F ^1D ^1P

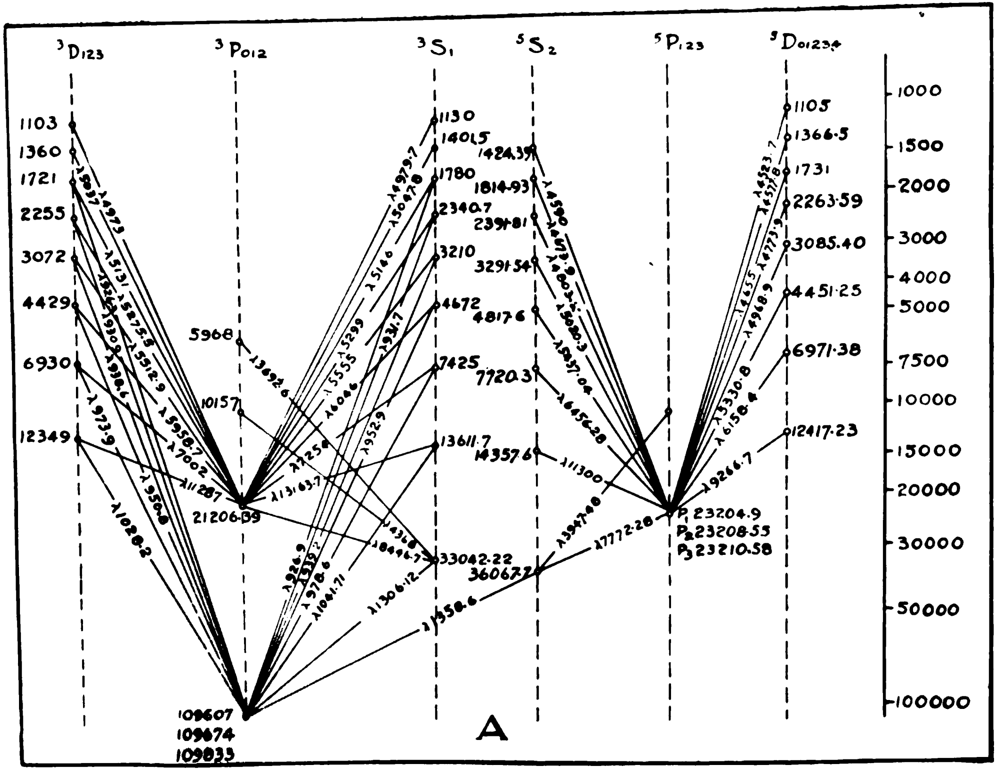

FIG. 19.—The Spectral Terms and Wave Lengths of the Triplet Quintet System of Atomic Oxygen.

of levels, in addition to those shown in Fig. 19, are possible for the atoms of oxygen, and that as a consequence we should expect to obtain, under suitable conditions, a set of arc lines for oxygen that could be embraced by two additional schemes of spectral terms and spectral series. Each of these schemes would consist of singlets and of terms of triple multiplicity. The term levels, or the spectral terms for the two systems corresponding to them, are given in Divisions B and C of Table II. These spectral terms are shown plotted according to the order of their magnitudes in Figs. 20 and 21. As yet no wave lengths are known in the arc spectrum of oxygen that are based on either of these schemes B and C, and one of the problems of the future will be to find whether there are any such wave lengths or not. It is too soon to state definitely that the wave length

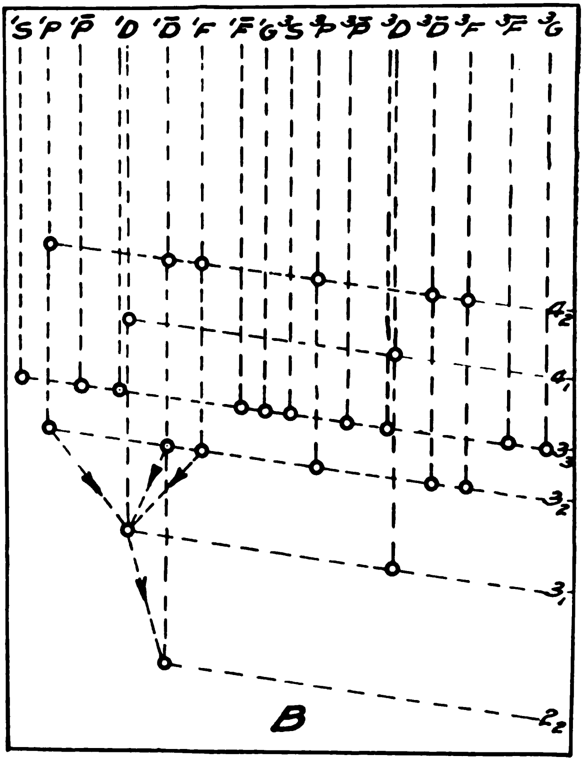

FIG. 20.—A New Singlet-Triplet System of Spectral Terms worked out for Oxygen on the basis of the Heisenberg-Hund Theory.

λ 5577·35 Å can be fitted into either of them, but as the normal triplet character of the Zeeman effect, shown by this wave length, suggests that the line probably originates in an electronic transition between levels of the singlet type, it is clear that while the line cannot be based on the terms in scheme A, it is quite possible for it to be one of the constituent members of the wave length system based on the terms of scheme B or of those of scheme C. Of the

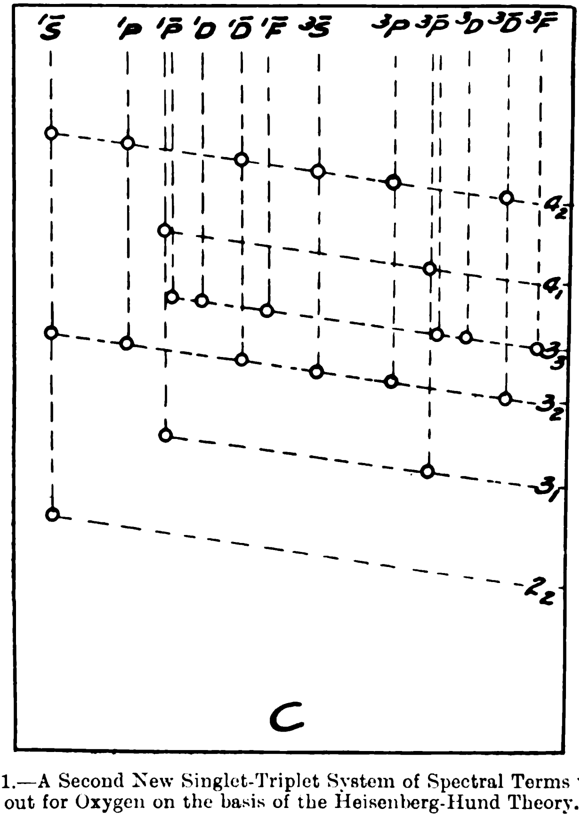

FIG. 21.—A Second New Singlet-Triplet System of Spectral Terms worked out for Oxygen on the basis of the Heisenberg-Hund Theory.

two schemes B is the likelier one on which to try to base the wave length λ 5577·35 Å. Electronic transitions between atomic levels that might give rise to the line are:—$^1P \sim {}^1S$, $^1D \sim {}^1P$, $^1F \sim {}^1D$, $^1G \sim {}^1D$, $^1\bar{P} \sim {}^1P$, $^1\bar{D} \sim {}^1D$, $^1\bar{F} \sim {}^1F$ and $^1\bar{G} \sim {}^1G$, etc. All these are provided for in both the schemes B and C. It is probable, however, that we shall be able to show that the spectrum represented by the terms in scheme B is the one that oxygen atoms can be made to exhibit with the least possible stimulation greater than that required to cause them to emit the spectrum represented by the terms in scheme A. We might, therefore, expect the auroral line to belong to this scheme. The term $^1\bar{D}$ is deepest in the scheme B, and the radiation corresponding to the electric transition $^1D \rightarrow {}^1\bar{D}$ is likely to give the strongest spectral line in the spectrum represented by the scheme. There is evidence, however, which indicates that the wave length of this radiation will be much shorter than λ 5577·35 Å. If I may be permitted to indulge in speculation, I would venture to express the view that of all the transitions provided for in scheme B, one of the three, $^1F_3 \rightarrow {}^1D_2$, $^1\bar{D}_2 \rightarrow {}^1D_2$, $^1P_1 \rightarrow {}^1D_2$, indicated by full lines in the diagram, will probably be found to be one that possesses features that would make it the most likely one for the production of the radiation that gives rise to the famous auroral green spectral line λ 5577·35 Å.

14. *Conclusion.*—From the information gained in a study of the auroral spectrum, it would appear that in the upper atmosphere, in the region between an altitude of 80 kilometres and that of 150 kilometres, there exist both nitrogen and oxygen, the evidence for the existence of the former being supplied by the appearance of the negative bands λ 4708 Å, λ 4278 Å and λ 3914 Å in the auroral spectrum, and that for the existence of the latter being furnished by the appearance of the famous green line λ 5577·35 Å in the same spectrum. While there is every reason for supposing that the nitrogen in the auroral regions exists practically in the same molecular form as it does at the surface of the earth and in our laboratories, the indications are that while oxygen is also present in the auroral regions it can exist in an atomic form there that can be reproduced at will in our laboratories, but that is somewhat different from the common atomic form in which we ordinarily find oxygen.

As there is ample evidence that ozone, too, exists in the upper atmosphere at auroral altitudes, and also evidence that such ozone undergoes dissociation at these altitudes into oxygen, it may very well be that the atomic form of oxygen required for the emission of the radiation of the auroral green line is one of the products of such dissociation.

[J. C. M'L.]

WEEKLY EVENING MEETING,

Friday, June 18, 1926.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Vice-President, in the Chair.

SETON GORDON, B.A. (Oxon.), F.Z.S. M.B.O.U.

The Golden Eagle and its Neighbours.

FIFTY years ago there were two species of eagles in Scotland, namely, the sea eagle and the golden eagle. But the sea eagle, or as it is sometimes called the erne, is extinct, although not so very long ago almost every rocky headland in the Isle of Skye had its pair of nesting sea eagles.

There is an old Gaelic rhyme :—

> Thrice the age of a dog the age of a horse,
> Thrice the age of a horse the age of a man,
> Thrice the age of a man the age of a stag,
> Thrice the age of a stag the age of an eagle,
> Thrice the age of an eagle the age of an oak tree.

There is, I think, no doubt that the eagle lives longer than most living creatures, or rather, perhaps I ought to say *would* live, because I think very few eagles ever die of old age. There are too many poisoned carcases on the moors ; too many traps ; too many keepers watching its eyrie with a gun, to give it much chance of finishing its long life. There is, I understand, a record of a eagle being kept 104 years in captivity in Vienna.

You may care to hear something about the position of the golden eagle in Scotland at the present day. I do not think there is any bird which suffers so much at the hands of egg collectors as the golden eagle. I will give you only one instance, but it is the same story everywhere.

The pair of golden eagles which I will show you to-night in their eyrie commenced to re-line their nest this year in the first week of February (the photographs were taken last year). By the middle of March the hen was sitting on two eggs, and at the very end of April, just when the eggs should have been on the point of hatching, an egg collector and his friend bribed a couple of men to climb the tree and steal the eggs for them. This was done although the golden eagle's eggs are protected by law, and although my hiding place was built up before the nest, ready to photograph the eagles directly the eggs should hatch.

The recognised price for a collector to pay a keeper or stalker for the eggs of the eagle is five pounds. A certain class of collector is not content with one clutch of eagle's eggs, but each spring sets out to rob every eyrie he possibly can.

People ask me whether the golden eagle is in any danger of extinction at the present day. It is not. But then neither was the sea eagle in any apparent danger of extinction seventy years ago; neither was the osprey in any apparent danger. The kite, too, is extinct in the highlands. Think how numerous it was a hundred and fifty years ago.

Sea eagle, osprey, kite and goshawk are four beautiful birds which have gone from the highlands of Scotland, never to return.

I am going to show you to-night photographs of the home life of the golden eagle as seen from a hide ten yards from the eyrie. In order to study and photograph at close quarters a bird as wary as the golden eagle the greatest care was necessary in the construction of the hiding place so as not to alarm the birds. It is easy to make a golden eagle desert her eggs, and so we were careful not to attempt any photography until the eaglets were hatched. At intervals of some days we placed a few fir branches and bunches of heather in position, and when it was considered that the eagles had become thoroughly accustomed to this new feature of the landscape a small hiding tent was put up on the spot and was carefully covered over with the branches and heather so that no canvas showed through. The photographs which I am going to show you to-night were taken partly by me and partly by my wife during our watch for upwards of 300 hours in our "hide." The short film of the eagles at the nest was also taken by her, and I am quite sure that none of the pictures could have been taken without her help.

Before describing the home-life of the eagles I should like to say a few general remarks on their habits. Each pair of eagles has two, sometimes three, eyries. An eyrie is very rarely used two years in succession. The eaglets remain eleven weeks in the eyrie, and the accumulation of the remains of hares, rabbits, grouse, and other prey become very high towards the close of the time. And so the parent eagles give the eyrie a rest the next spring so that the summer sun may thoroughly cleanse the nest.

Some of the Scottish eyries must have been used for at least half a century, probably much longer. Most of these old-established eyries are well known, and the only chance an eagle has of outwitting the collectors is to leave the ancestral homes and build a new eyrie. An eagle nests either in a tree or on a rock. In the central highlands a tree is frequently chosen, but a rock seems always to be used by the eagles of the west. I have seen eyries in a Scots fir and in a birch.

The golden eagle lays usually two eggs. They are about the size of a goose's egg, and the first-laid egg is usually marked thickly with red-brown spots. She sits on them for five weeks. It is possible that the cock shares in the duties of hatching the eggs, but I do not think so. One day this April while I was watching the eyrie that was robbed shortly afterwards the cock flew up and settled on a branch a few yards from where his mate was sitting. I hoped I might see the "change over" between cock and hen, but after satisfying himself that all was well with his mate he shook himself like a dog (there had been a heavy hail shower a few minutes before), spread his great wings, and was gone.

I think the eagle is unique among British birds in that it never lays again if its eggs are robbed. The golden eagle is very fastidious in her choice of building materials. The eyrie may be 6 to 8 feet in diameter, and the crown of the eyrie is made of green pine branches which the eagle breaks off the growing trees with her strong bill. The lining is almost always made of the great wood rush—*Luzula sylvatica*—and the eagles fly long distances to find it, or a pine tree, to pull branches from.

It is curious that there should be a wide-spread belief that the golden eagle attacks anyone venturing near its eyrie. As a matter of fact the eagle is an unusually shy bird, and when disturbed at the eyrie disappears from view.

I am sure you have all heard stories of eagles taking young children. A golden eagle can lift a mountain hare without difficulty, and I should say that the weight of a newly-born child is approximately the same as the weight of a full-grown mountain hare.

But when you investigate those stories of eagles carrying off children you find that most of them are myths. But I will tell you one interesting story. There is an old lady now living in the Isle of Skye who tells me that her grandfather when an infant was carried by an eagle across an arm of the sea loch at Dunvegan. Some people working in the fields near saw that the eagle had alighted with something unusual in its talons. They ran up and drove the bird away, and the baby was found to be unhurt.

This photograph of the cock eagle at the eyrie represents the results of some eighteen hours' continuous watch in the hide. It was the first picture we succeeded in taking, and to my mind it is the finest of them all, because it shows the cock eagle in a truly magnificent pose. As he stands there, full of grace and very proud, you can understand why from earliest times the eagle has been called the king of birds. He has just deposited a plucked grouse in the eyrie. But you may care to hear a little of the difficulties we had in getting this one picture. After the hide had been carefully camouflaged I entered it in the morning with my camera and took the first day's watch. In the front of the hiding tent a small hole was cut, and through this hole the lens of the camera projected. The lens and its concealment is always a problem, for all birds hate to see a great eye staring at them. Above the lens a small hole was cut to serve as a peep-hole. During the first watch of six hours I discovered that the peep-hole was so small that it was quite impossible to keep constant watch. I suffered severely from a complaint we have named "peep-hole eye." A peep-hole in a hide is so small that there is only room for one eye, and after a time the eye becomes very painful from excessive work. The first day I saw nothing. The second day my wife took the watch, but had no better fortune, for the eagles seemed to have their suspicions of the "hide." But although we saw nothing of the old birds the eaglets were most interesting to watch. The young hen eaglet was a thoroughly objectionable bird. Every now and again she attacked her brother, and drove him round and round the eyrie. The attack was always entirely unprovoked. The cock eaglet would be lying asleep. As though suddenly making up her mind the hen eaglet would walk unsteadily over to him and aim a deliberate blow at his "behind." The unfortunate victim would rise to his feet and run for safety, and it was only his superior speed that saved his life.

Usually the blows were aimed at his body, but sometimes at his neck and head, and we feared that each day would be his last. And so we named the young hen Cain and the young cock Abel. Cain pulled out much of her brother's down, and this down strewed the heather to a distance of quite fifty yards from the nest as it was carried there on the strong west wind that blew most days. In this slide you see Cain with a large piece of Abel's down. She was bothered with this down quite a lot, for it stuck to her bill and she could not get it off anyhow! So she suffered for her bullying! Sometimes, after almost exhausting herself pecking her smaller and unfortunate brother, Cain stood up in the eyrie and flapped her downy stumps while she uttered a wild yell of defiance. It was an extraordinary and weird cry and I shall never forget it.

An eagle usually hatches two chicks. One of them often disappears mysteriously. I have now no doubt that one of the chicks kills the other. But although Cain delighted in making Abel's life a misery to him she did not seem to wish to lose him, for one day in his efforts to escape he almost fell out of the nest. My wife was on watch in the hide at the time, and just as Abel was disappearing over the edge she saw Cain with an effort snatch him back and almost throw him (for she was much stronger than he was) back on to the platform of the nest.

On one occasion the mother eagle returned to the eyrie just in the midst of a brutal attack on Abel by his bullying sister. Poor Abel at the moment was having a real bad time of it, but his mother stood and calmly watched the down flying from her unfortunate son. She probably realised that it was better for him to be brought up in a spartan school. At all events Abel survived, and after a few weeks Cain ceased bullying her brother.

In this slide you see the eaglets lying together quietly in the eyrie. On the outside of the eyrie are the remains of a jackdaw. A great many people believe that eaglets live almost entirely upon grouse. As a matter of fact an eagle's diet is very varied. This particular pair of eagles hunted and caught squirrels, and brought them to the eyrie for their young. It was comical to watch the eaglets trying to swallow pieces of squirrel. The long fur stuck in the throat, and it was delightful to see the disgusted expression of a young eagle as he tried to swallow a squirrel's tail—mostly dry fur with very little meat inside it. I have seen a stoat in an eagle's eyrie, and a friend of mine once found a raven. Eagles do, of course, take grouse, but I am quite certain that if mountain hares and grouse are together on a moor the eagles always take the hares in preference to the grouse. They are almost equally fond of rabbits. The cock eagle was a magnificent bird. He was much lighter in plumage and considerably smaller than the hen. He was dashing and debonair—the beau-ideal of a gentleman. The gleam of his eye was magnificent. I shall never forget one hot sunny afternoon putting my head half out of the hiding tent to get a breath of air. I saw against the blue of the sky a small dark speck which I thought at first was an insect, but with incredible speed the small speck grew in size until I saw that it was the cock eagle planing downwards from the high snowy corries behind. He was travelling like a thunderbolt. In one claw he held a ptarmigan, and this extra weight no doubt increased his speed. I expect I shall be accused of exaggeration when I say that his speed must have been at least two hundred miles an hour, but I am confident that it is no over-statement. I have seen nothing in the bird world to equal that splendid downward rush, and to see it was worth many scores of hours of patient watching. Regularly about three o'clock the cock eagle brought prey to the eyrie. He laid it there, stood perhaps for a minute looking indifferently at his shrieking family, then spread his great wings and sprang into space. The hen eagle evidently watched him from a neighbouring tree, for almost at once she glided down to the nest and commenced to feed the family on the food left by the cock. When the eaglets were young she gave Cain much more than her fair share of food. Cain's lung power was far ahead of Abel's, and as he always was "on the spot" it was not until Cain's crop was bulging that she turned her attention to Abel, who usually lay dejectedly at the bottom of the eyrie. One evening she fed Cain for three-quarters of an hour and you will see from this photograph how Cain's crop is bulging out. Cain began to tear food up for herself long before Abel could do so. Sometimes Abel lay so miserably in the eyrie that he made no effort to call for food even when his sister was being fed, and then his mother seemed to take pity on him. She leaned over towards him and offered him some grouse tit-bits. Like a peevish and ailing child, he took from her bill some half-dozen mouthfuls of grouse, then lay down in the nest and refused more food. We only once saw the hen eagle bring prey to the eyrie ; the cock did all the hunting, but very rarely fed the eaglets. But at seven o'clock one morning, when my wife was in the hide, the cock did come and feed the young, and she got an exceptionally clear photograph of him transferring a small piece of grouse to the eaglet's bill.

As one watched the eagles on the lonely hillside it was interesting to listen to the other birds of the forest. A redstart used to sing on the top of our hide. Other birds that we heard were the cuckoo, the crested tit, the goldcrest, the missel thrush, the wren, the chaffinch, the willow warbler, and the song thrush. On fine sunny days it was a pleasant and very fascinating experience to remain motionless hour after hour at the hide watching the home-life of the golden eagles. But there were days of storm when the west wind brought with it wild squalls of hail which pattered like small shot against the hide. The eagle's tree rocked in the wind, and the mother eagle as she guarded her young swayed gently to and fro, keeping her balance as a sailor does in heavy weather. Sometimes after a long watch we were so stiff that we could scarcely crawl from the hide and stand erect. We were never, of course, in the hide together, but once or twice our collie dog Dileas shared our watch, or, to be more correct, she slept while we watched. You must remember that it was necessary for whoever was in the hide to remain absolutely quiet ; the least sound or movement might have been fatal.

One morning the cock eagle arrived at the eyrie carrying a roe-deer calf in one claw. The calf was complete except for the head and the entrails, and the weight was so great that the cock had to have several rests on his journey up from the low ground. When he arrived he threw down his burden and stood panting for a moment. He then crossed to the other side of the eyrie and commenced to feed himself on an old grouse. Cain at once pounced on the roe calf with loud yells of triumph, using those same wild cries as we heard after her attacks on poor Abel. Abel, too, was full of excitement at this unexpected and welcome addition to the larder, and picked a feather up from the floor of the eyrie and dashed about with it in his mouth.

The eaglets, of course, expected to be fed, but to their disgust their father insulted them by offering them tiny pieces of a grouse that was already in the eyrie. The eaglets plainly felt the injustice of it all keenly ; but what could they do ?

Day after day we hoped to be able to photograph both parent eagles at the eyrie, but it was not, as you will see, until the young eaglets were ready to fly that we succeeded in doing this. Time and again the hen was near when the cock arrived with food, but she always waited until he had gone before coming on to the nest. However, one evening, as she was looking out over the corrie, the cock alighted at the eyrie, and you see him laying a piece of heather in the nest. The eagles were particular to keep the appearance of the eyrie smart, and often brought green pine branches or fresh heather bunches.

A curious thing happened one night. There were in the nest at the time a hare and two grouse. The mother eagle arrived at the nest and the young cheeped expectantly, for they thought she would feed them. But instead she set about clearing the eyrie of food. She first removed the grouse and then the hare. The hare was firmly fixed in the eyrie, and when she lifted it she carried away with it a number of sticks, so that as she sailed across the hillside with her burden she looked very like an airship.

Each pair of golden eagles are supposed to have a larder near the eyrie, and I have no doubt she was carrying off the prey to the larder. The eaglets hated seeing their food supply being taken from them.

When the eaglets were almost ready to fly Cain was standing at the edge of the eyrie one day when the edge of the nest gave way and she fell to the ground along with a roe-deer calf and a grouse. She was frightened but unhurt, and walked away through the heather. I did not see her again.

The parent eagles now considered that it was time for Abel to leave the nest. They gradually reduced his rations, and for the last few days did not feed him at all. He became starving, and was so weak that he was scarcely able to call for food. At last, on the morning of July 16 he was gone. During the last few days of its stay in the eyrie the eagle practised wing exercises, and also practised grabbing exercises. Sometimes he flapped his wings so hard that he almost flew himself out of the nest.

[S. G.]

GENERAL MONTHLY MEETING,

Monday, July 5, 1926.

Sir Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S.,
Treasurer and Vice-President, in the Chair.

Colonel Nicholas Timothy Belaiew, C.B., F.C.S.

was elected a Member.

The Special Thanks of the Members were returned to Professor J. C. M'Lennan, LL.D. D.Sc. F.R.S. M.R.I., for his Donation of Twelve Pounds to the Research Fund; and to Mr. Bence Jones for his Gift of an Original Letter from Mrs. Faraday.

The following Presents received since the last Meeting were laid on the table and the Thanks of the Members were returned for the same :—

FROM

Hind, A. M. (the Author)—Claude Lorrain and Modern Art (Rede Lecture, 1926). 8vo. 1926.

King, Professor L. V. (the Author)—Gyromagnetic Electrons. 8vo. 1926.

Parker, W. Rushton, Esq., M.D. M.R.I.—The Tomb of Huy. By N. de G. Davies and A. H. Gardiner. (Theban Tombs Series 4.) 4to. 1926.

Petra: Its History and Monuments. By Sir Alexander Kennedy. 4to. 1925.

MEETING OF MEMBERS,

Tuesday, July 20, 1926.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G.,
President, in the Chair.

Presentation to Sir James Crichton-Browne on his Retirement from the Office of Treasurer.

SIR ERNEST MOON said :—

Your Grace, Ladies and Gentlemen, you all know, I imagine, the regrettable event that has taken place with regard to the management of this Institution. The event I refer to is the fact of the resignation of our Treasurer, Sir James Crichton-Browne; but I think that the term Treasurer of this Institution does not suggest the right functions which he fulfils. One regards a treasurer as being a person who manages the finance of a society, or company or whatever it may be. Sir James Crichton-Browne is not only a Chancellor of the Exchequer, he is also the Prime Minister here. He has acted in that capacity for no less than 37 years. He has throughout acted with devotion and wholeheartedness in performing the duties which have fallen upon his shoulders, and when the announcement came of his retirement there was a widespread feeling that something ought to be done in the form of a commemoration of that event. Now I am not here for the purpose of appraising the services which have been rendered to the Institution by Sir James Crichton-Browne; I am here for a different purpose. As I say, there was a widespread feeling—I think we may call it an epidemic—amongst the members of the Institution that something should be done, and it was decided that there should be a presentation made to Sir James Crichton-Browne which would be in a measure a token of the appreciation of his services which was felt by the members, and which also would be a memorial which would remind him of his association with the Institution, and also, I hope, remind him of the very many friends whom he has made here.

Accordingly a committee was appointed; I think I may also call it, having regard to Sir James's country, a commit*tee*, for nothing is ever done in Scotland without a commit*tee*. A committee was, therefore, appointed for the purpose of considering what form such a memorial should take, and they pursued their deliberations. But then there arose a very strenuous opposition from a quite unexpected corner. Somehow or other Sir James himself found out what was going on, and he, with the utmost vehemence—with a vehemence which we all know he has—he denounced the whole proceeding, and the argument was this: he said it was not right, that at most he had done but his duty, and that in his case the duty had been a pleasure, and that he had enjoyed the task so much that this was almost immoral. While the committee listened to Sir James's dissertation, I think you will agree they took the right course, they went on their own way and the result of this is that I have been asked, as an old member of the Institution (for I have been a member since 1884, and am at present a Member of the Board of Managers) to invite the Duke of Northumberland as the President of the Royal Institution to present the testimonial (I do not like the word testimonial), the memorial to Sir James which has been prepared for him. I think I ought to assure the Duke that if any personal violence is threatened, some member of the Institution will come to his assistance, but I hope that Sir James will receive the gift peacefully. Your Grace, I have now to ask you to present this memorial to Sir James Crichton-Browne.

THE DUKE OF NORTHUMBERLAND, the President, said :—

Sir James Crichton-Browne, Ladies and Gentlemen, I cannot say how much pleasure it gives me to be present on this occasion, and to be called upon to make this presentation to Sir James, and I hope that it will not prove so dangerous a task as Sir Ernest Moon appears to imagine.

Ladies and Gentlemen, I do not know that I can add very much to the very eloquent and, if I may say so, felicitous speech which Sir Ernest Moon has just made. With regard to the great services which Sir James has rendered to this Institution, I think that perhaps the most striking testimony of all as to the services which he has rendered as Treasurer are the figures showing the endowment fund of this Institution as they stood in 1889, when he became Treasurer, and as they are to-day. In 1889 the funds for the endowment of this Institution were £22,734; to-day they amount to no less than £112,500. If this is not a testimony to the financial ability of the Treasurer, I do not know what it is. Similarly the endowment fund for the Davy Faraday Research Laboratory has increased from £62,000 in 1896 to £88,295. But in addition to these services which he has rendered in his capacity as Treasurer, there have been other very valuable services which we should not forget. On two occasions he has delivered a Friday Evening Discourse; he has represented the Institution on many very important occasions, notably at the Stokes Jubilation Celebration and at the Glasgow University Celebration in 1901. He has also delivered several important speeches on occasions connected with the Royal Institution, notably on the decease of Dr. John Tyndall and Sir Frederick Bramwell, and also at the Centenary Commemoration; and on every occasion when he has appeared on the public platform he has delighted his audiences with a fire and felicity of expression which unfortunately are very rare in these days and are reminiscent of the age of Gladstone and Disraeli. He is reminiscent of an earlier age, but I can assure you that I do not mean that he is in any sense of the word antiquated, for he appears to possess the secret of perpetual youth, and although he still retains the best traditions of the Victorian Age he keeps as up-to-date as anyone.

When we remember that all this work has been performed for the Royal Institution in the intervals of a very busy life, that he was for thirty-seven years Lord Chancellor Visitor in Lunacy, that he was for many years President of the Medical Society and other societies, that he has been the author of many very important works, and that in spite of all these more or less professional undertakings he has participated in public life and taken an active interest in many public questions—I think that, when we remember all this, I think we can realise what we owe to him. But it seems to me that there has been one motive that has actuated him above all others, and that is a love of and zeal for the truth and a determination to uphold the principle which he believes regardless of all consequences, and I do not think that anyone can pay a higher tribute to any man than to say that.

But, in addition to all this, there is something else that I should like to mention on this occasion. His services to us have not only been connected with his purely financial duties as Treasurer, he has brought to the services of the Institution certain qualities; these are the qualities of good judgment and tact, and it is not too much to say that they have brought us through many difficult places, and have steered the ship through many troubled waters. In this respect I do not think that we shall ever know what we owe; we can only guess it. But I should like to assure him in your name that we do appreciate those more or less unseen and invisible qualities: we realise that in him we have that rare combination of devotion and efficiency combined with sound judgment and unfailing tact, and above all that invaluable quality of inspiring friendship in all those with whom he comes in contact. Now, Sir James Crichton-Browne, I have very much pleasure in presenting you on behalf of the Institution with this memento of the services of the Institution and of the affection with which we regard you.

SIR JAMES CRICHTON-BROWNE, in reply, said:—

Your Grace, Ladies and Gentlemen, this is one of those occasions in the lifetime of a man which is designed to gratify, and it *does* gratify immensely. At the same time it summons up a host of old memories, many of them inevitable, and it evokes emotions that are stirred up in me by this ceremony. At the moment it is an overwhelming sense of this company of my deserts and the recognition of my duties in such kind and generous words. I would say that ever since I became a member of the Royal Institution forty-six years ago I have been incurring an ever-increasing debt of gratitude towards you, and any small services I have been able to render in my capacity as Treasurer have been requited over and over again. And in many ways it is always a pleasure to receive money on behalf of any object in which one is interested, and it has been my agreeable duty to sit at the receipt of customs here for thirty-seven years, seeing the funded property of the Institution increase from £23,000, which it was when I took office in 1889, to £112,000, at which it stands to-day. But all the time the Royal Institution has had to bear the cost of £8,000 or £9,000, £10,000 a year nearly, carrying on its educational work and making possible those researches, those memorable researches, which have taken place and no assistance has been required. Only once have I had to exercise any solicitations. That was when Sir Frederick Abel sent me down to the country to interview an old lady who had a benevolent disposition. By my glowing account of the Royal Institution which she had never seen, I succeeded in persuading her to give a gift of £10,000, and also a bequest of another £10,000 in her will. I hope the money will continue to flow in, for I foresee that it will be wanted more than ever, and I venture to predict that it will not only add to the advancement of pure science, but will also promote discoveries which will help the industrial prosperity of the country. Would the nation stand exactly where it does to-day if the Royal Institution had not taken an interest in magnetic electric induction and liquid air?

The Treasurer of the Institution, as I have no doubt my successor will find, has to take part in the general work of the Institution, and it is taking part in that work that I owe many of my obligations to the Royal Institution, for I have been able to form a circle of friends of the right sort, well-informed friends, good friends, staunch friends, many of whom are notabilities in the domain of science, and many of whom are here to-day and have given me token of their unabated kindness, goodwill and sympathy towards me. The Treasurer of the Royal Institution comes in contact more or less with every branch of science other than his own, and I am inclined to ascribe one of my obligations to the Royal Institution, that is to say, a not unusual degree of vigour that I may have retained at an advanced age, and I am inclined to attribute this to the Royal Institution. The popular formula is that a man is as old as his arteries, but I believe that a man is as old as his nerves and their great functionary the brain that presides through the whole of life, and not only the arteries but other tissues of the body; and if the nerves of the brain and their functional property are to retain their vigour and control, then they must receive the proper nutriment, and it is this nourishment, this intellectual nourishment, which the nerves need, and which the Royal

Institution provides for its members. If the members have this nourishment then there will be no danger of the totally unnecessary condition of senility. I hope that the members of the Royal Institution will bear that in mind, for it seems to me that membership of the Royal Institution is a sort of patent of longevity, and a life insurance policy of the grand old age; and if the members will impress that upon their friends also, they will realise what a hazardous thing it is to resign from the Royal Institution. While acting as Treasurer I have served under three Presidents, and they have all been Dukes of Northumberland. I have never appealed in vain on behalf of the Royal Institution to any one of these Dukes of Northumberland without receiving good advice or substantial assistance. I have just been recalling, Your Grace, that in the early days of electric lighting your grandfather introduced it throughout the whole of the Royal Institution at his own cost, and I hope that there will always be Dukes of Northumberland to fill the Chair here.

During the 127 years of the Society's existence Dukes of Northumberland have for 75 years occupied the Chair, and I hope there will always be Dukes of Northumberland to fill these vacancies as they occur.

The Royal Institution has passed through four epochs: there was the epoch of Davy, which, of course, included Rumford and Thomas Young, extending from 1790 to 1823; there was the epoch of Faraday extending from 1823–67; there was the Tyndall epoch from 1867–87, and there was the epoch of Dewar extending from 1887 to 1923. It is with the epoch of Dewar that I have been associated, and I am proud to think that I enjoyed the intimate friendship of that great man during all the years of his strenuous work. The Royal Institution under his vigorous and energetic guidance became a centre of science, and more than that those who have seen the generous influence of Lady Dewar will recall many delightful meetings, and we are all very grateful to Lady Dewar for what she did for the Royal Institution. But I was going to say that I had recently seen a picture of the Royal Institution, of this very theatre crowded with listeners who flocked to hear the expositions of Tyndall dealing with heat and radiation and to the striking demonstrations of Dewar on the properties of matter at low temperatures. I can recall many a time in the 'eighties and 'nineties of last century when at Dewar's lectures many of the would-be auditors were sent away all eager to hear about liquid air and liquid hydrogen.

When, Ladies and Gentlemen, the Royal Institution has entered upon a new epoch, a fifth epoch of Sir William Bragg, if I might indulge in a little crystal-gazing, I might say that I see a renewal of the glorious prosperity of the Royal Institution. The Royal Institution will, I am confident, maintain its popularity, extend its usefulness and throw new light upon the properties of matter. You must pardon me for a little retrospection which is probably well known to all of you, but I have been obliged to do this; because, being identified with the Royal Institution in the peculiarly brilliant era of its history, you, looking at that, have become greatly dazzled and have likewise magnified the small part that I have played in it.

I am a victim to medical authority, and my doctor has told me that I must resign some of my appointments and take more rest, and so amongst other things I have laid aside the Treasurership of the Royal Institution, but did not do it all in accordance with the doctor's orders, but because I feel that the time has come when it would be for the benefit of the Royal Institution that it should have younger blood infused in the office of the Treasury. I am indeed an extinct volcano—but not quite, because it really turns out I have only changed my seat. I am still a Manager, and I hope that while the light lasts I may still be able to do it some small service. I am very gratified that I am succeeded by my friend, Sir Arthur Keith: he is a man of wide scientific interest and scientific attainment: he possesses great versatility, thoroughness and suavity. Pardon me, Ladies and Gentlemen, for keeping you so long, but you must allow me to say again how I thank you for this memento and the kind manner in which it has been conveyed to me as a present from the Members of the Royal Institution.

SIR WILLIAM BRAGG said:—

Before this very memorable and very happy gathering of friends breaks up I should like to express our gratitude to His Grace for presiding this afternoon. You have heard much of the researches and the work done here, and I must add that it would do your hearts good sometimes to hear what is said by visitors from Overseas when they come here to a place they most wish to see when they have a love of science. But the Royal Institution does not benefit solely by great men who devote their labours and time to the services of science; it is not only to those who have done this, but to those who have served as secretaries, treasurers, etc., that it owes a debt, and it is in the very friendly atmosphere that has been created that the great work has been done.

We are very glad to see the Duke of Northumberland in the Chair, and I can assure him on your behalf that we heartily appreciate his kindness in coming here this afternoon.

THE DUKE OF NORTHUMBERLAND replied:—

Ladies and Gentlemen, I am extremely grateful to the very kind way in which you thank me. It has been the greatest pleasure to me to be present here. I have been associated, not very long it is true, but for some eight years with Sir James Crichton-Browne, and nobody could have been more kind and more helpful to me. When I first came here I felt a little trepidation because I felt that I had hardly an equal scientific knowledge with some of its members, and also I felt some hesitation in being associated with so eminent men of science as Sir James Crichton-Browne and Sir James Dewar; but as Sir William Bragg said there is also a very friendly atmosphere in this Institution, and instead of meeting great scientific notabilities I have only met friends. It is a great privilege to be President of this Institution, but it is an even greater privilege to be able tc make so many friends.

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21 ALBEMARLE STREET, LONDON, W.1

October 1927

ROYAL INSTITUTION OF GREAT BRITAIN.

GENERAL MONTHLY MEETING,

Monday, November 1, 1926.

JOHN MITCHELL BRUCE, C.V.O. M.D. LL.D. F.R.C.P.,
Manager and Vice-President, in the Chair.

Llewelyn Birchell Atkinson,
George E. K. Blythe,
Oswald Cox,
Alexander Thomas Fraser, M.B.
Captain C. W. Hume,
Edward Preston, B.Sc.

were elected Members.

The Special Thanks of the Members were returned to Mr. Sidney G. Brown, F.R.S. M.R.I., for his present of a Microphone Amplifier.

The following Lecture Arrangements for November and December, 1926, were announced:—

G. W. C. KAYE, Esq., O.B.E. M.A. D.Sc. M.R.I., Superintendent of the Physics Department, The National Physical Laboratory. Three Lectures on THE ACOUSTICS OF PUBLIC BUILDINGS. On *Tuesdays*, Nov. 2, 9, 16. (The Tyndall Lectures.)

SIR WILLIAM BRAGG, K.B.E. LL.D. F.R.S. M.R.I., Fullerian Professor of Chemistry. Four Lectures on THE IMPERFECT CRYSTALLISATION OF COMMON THINGS. On *Tuesdays*, Nov. 23, 30, Dec. 7, 14.

SIR EDGEWORTH DAVID, K.B.E. C.M.G. D.S.O. F.R.S. Two Lectures on ANTARCTIC EXPLORATION PAST AND FUTURE. On *Thursdays*, Nov. 4, 11.

ROBERT R. MARETT, Esq., M.A. D.Sc., Fellow and Tutor of Exeter College, Oxford, and University Reader in Social Anthropology. Three Lectures on THE ARCHÆOLOGY OF THE CHANNEL ISLANDS: 1. PALÆOLITHIC PERIOD; 2, 3. NEOLITHIC AND BRONZE AGES. On *Thursdays*, Nov. 18, 25, Dec. 2.

SIR SQUIRE SPRIGGE, M.A. M.D. B.C. F.R.C.S., Editor of the "Lancet." Two Lectures on 1. EARLY MEDICAL LITERATURE; 2. MEDICAL LITERATURE IN RELATION TO JOURNALISM. On *Thursdays*, Dec. 9, 16.

REV. E. M. WALKER, LL.D., Pro-Provost of Queen's College, Oxford. Three Lectures on THE STUDY OF HISTORY. On *Saturdays*, Nov. 6, 13, 20.

GEORGE C. SIMPSON, Esq., D.Sc. F.R.S. M.R.I., Director of the Meteorological Office. Two Lectures on ATMOSPHERIC ELECTRICITY. On *Saturdays*, Nov. 27, Dec. 4.

CYRIL ROOTHAM, Esq., M.A. Mus.Doc., Organist, St. John's College, Cambridge. Two Lectures (with Musical Illustrations) on HENRY PURCELL AND HIS CONTEMPORARIES. On *Saturdays*, Dec. 11, 18.

PROFESSOR A. V. HILL, O.B.E. M.A. F.R.S. Six Lectures (Illustrated), adapted to a Juvenile Auditory, on NERVES AND MUSCLES: HOW WE FEEL AND MOVE: 1. NERVES AND THE MESSAGES THEY CARRY; 2. MUSCLES AND HOW THEY MOVE; 3. THE HEART AND SOME OTHER MUSCLES; 4. THE LUNGS AND BLOOD: HOW THE MUSCLES GET AIR AND FUEL; 5. NERVES AND MUSCLES WORKING TOGETHER; 6. SPEED, STRENGTH AND ENDURANCE. On Dec. 28 (*Tuesday*), Dec. 30, 1926; Jan. 1, 4, 6, 8, 1927. (101st Course).

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Buchanan, The Executors of the late Dr. J. Y.—Collected Papers of J. Y Buchanan, Vol. I. 8vo. 1913.

Burlington Fine Arts Club—Catalogue of an Exhibition of late Elizabethan Art. 4to. 1926.

Carnegie, David, Esq., C.B.E. (the Author)—The History of Munitions Supply in Canada, 1914-18. 8vo. 1925.

Chamberlin, F., Esq., LL.B. M.R.I.—El Senor Chamberlin en Menorca. By T. H. Sanz. 8vo. 1926.

Hadfield, Sir Robert S., Bart., D.Sc. F.R S. M.R.I.—Address at Opening of new Laboratories at the 21st Anniversary Celebrations of Sheffield University, 1926. 8vo. 1926.

Harvard Institute for Tropical Medicine—Medical Report for the Hamilton Rice 7th Expedition to the Amazon, 1924-25. 4to. 1926.

Komatsu, Professor Shigeru (the Editor)—Mitsuru Kuhara's on the Beckmann Rearrangement. 8vo. 1926.

London, Corporation of, Library Committee—Classification of London Literature in the Guildhall Library. 8vo. 1926.

Maude, Aylmer (the Author)—Life of Marie C. Stopes. 8vo. 1924.

GENERAL MONTHLY MEETING,

Monday, December 6, 1926.

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S F.R.C.S.,
Treasurer and Vice-President, in the Chair.

William Henry Ballantyne, B.Sc.
Charles Vickery Drysdale, O.B.E. D.Sc.
Alfred Cecil Eborall,
Mrs. W. H. Eccles,
William Charles Edwards,
Commander James Orville Gawne, M.Sc. U.S.N.
Herbert Clifford Holder,
Miss Edith McLean,
Anthony John Mundella,
Carleton C. Murdock,
Ernest Alfred Notcutt,
Mata Prasad,
John Monteath Robertson,
Ernest William Rowntree, B.A.
Henry Gage Spicer, J.P.
William Andrew Shakespeare Vardy,
Charles Pennock Williamson,

were elected Members.

The Managers reported, That at their Meeting this day they had elected Julian Sorell Huxley, M.A., Fullerian Professor of Physiology for the term of three years, in conformity with the Deed of Endowment, the appointment dating from January 14, 1927.

The Special Thanks of the Members were returned to the Department of Scientific and Industrial Research for their valuable gift of the whole of the Apparatus and Equipment used by Sir William Bragg in his researches on the X-Ray Analysis of Crystals; and to Mr. Frank Scudder for his gift of a Crystal Model, nine Lantern Slides used by Sir Henry Roscoe in his lectures on Faraday, and a "Franklin" Medal in aluminium which belonged to Faraday.

The following Lecture Arrangements for Before Easter, 1927, were announced:—

R. WHYTLAW GRAY, Esq., O.B.E., Professor of Chemistry, University of Leeds. Two Lectures on SMOKES AS AERIAL DISPERSE SYSTEMS. On *Tuesdays*, Jan. 18, 25.

JULIAN S. HUXLEY, Esq., M.A., Professor of Zoology, King's College, London. Six Lectures on PROBLEMS OF ANIMAL GROWTH AND DEVELOPMENT. On *Tuesdays*, Feb. 1, 8, 16 (*Wednesday*), 22, March 1, 8.

GEORGE SHEARER, Esq., D.Sc. M.R.I., of the Davy Faraday Research Laboratory of the Royal Institution. Two Lectures on X-RAYS AND THE CHEMICAL MOLECULE. On *Tuesdays*, March 15, 22.

JOHN W. COBB, Esq., C.B.E. B.Sc., Livesey Professor of Coal Gas and Fuel Industries, University of Leeds. Two Lectures on SOME PROPERTIES OF COKE. On *Tuesdays*, March 29, April 5.

JOHN R. H. WEAVER, Esq., M.A., Fellow and Tutor of Trinity College, Oxford. Two Lectures on ROMANESQUE AND EARLY POINTED ARCHITECTURE IN SPAIN (11TH TO 13TH CENTURIES). On *Thursdays*, Jan. 20, 27.

SIR WILLIAM BRAGG, K.B.E. D.Sc. F.R.S. M.R.I., Fullerian Professor of Chemistry. Three Lectures on ACOUSTICAL PROBLEMS TREATED BY LORD RAYLEIGH: 1. ÆOLIAN TONES; 2. ECHOES; 3. BELLS. On *Thursdays*, Feb. 3, 10, 17.

JOHN GARSTANG, Esq., D.Sc. F.S.A., John Rankin Professor of Archæology, University of Liverpool. Three Lectures on THE PROGRESS OF HITTITE STUDIES. On *Thursdays*, Feb. 24, March 3, 10.

J. GUILD, Esq., A.R.C.Sc. F.R.A.S. F.Inst.P., of the National Physical Laboratory. Two Lectures on COLOUR MEASUREMENT AND STANDARDISATION. On *Thursdays*, March 17, 24.

HAROLD J. E. PEAKE, Esq., M.A. F.S.A., President of the Royal Anthropological Institute. Two Lectures on THE BEGINNINGS AND EARLY SPREAD OF AGRICULTURE. On *Thursdays*, March 31, April 7.

E. W. TRISTRAM, Esq., A.R.C.A. F.S.A., Professor, Royal College of Art, South Kensington. Two Lectures on ENGLISH MEDIÆVAL WALL-PAINTING: 1. EARLY; 2. LATE. On *Saturdays*, Jan. 22, 29.

WALTER DE LA MARE, Esq. Three Lectures on 1. CRAFTSMANSHIP IN VERSE; 2. "ATMOSPHERE" IN FICTION; 3. THE SUPERNATURAL IN FICTION. On *Saturdays*, Feb. 5, 12, 19.

JOHN B. MCEWEN, Esq., M.A. Mus.Doc. F.R.A.M., Principal of the Royal Academy of Music. Three Lectures (with Musical Illustrations) on BEETHOVEN. On *Saturdays*, Feb. 26, March 5, 12.

SIR ERNEST RUTHERFORD, O.M. LL.D. D.Sc. Pres.R.S. M.R.I., Professor of Natural Philosophy, R.I., and Cavendish Professor of Experimental Physics, University of Cambridge. Four Lectures on THE ALPHA RAYS AND THEIR APPLICATION TO ATOMIC STRUCTURE. On *Saturdays*, March 19, 26, April 2, 9.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

Bragg, Sir William, K.B.E. F.R.S. M.R.I. (*the Author*)—Old Trades and New Knowledge (Christmas Lectures, 1925-26). 8vo. 1926.

Coxworthy, Miss J.—Electrical Condition. By Franklin Coxworthy. 8vo. 1924.

Hale, Dr. G. E., Hon. M.R.I. (*the Author*)—Beyond the Milky Way. 8vo. 1926.

Joseph, L., Esq. (*the Author*)—What are we? 8vo. 1906.

Manchester Steam Users' Association—Boiler Explosions Acts, 1882 and 1896, Board of Trade Reports, Nos. 2713-2780. 4to. 1925.

Parker, W. Rushton, Esq., M.D. M.R.I.—Bushman Paintings—copied by H. M. Tongue. Preface by H. Balfour. 4to. 1909.

Watkins, A. Greenwood, Esq., M.R.I.—Merck's Annual Reports, 1926, Parts 1-2. 8vo.

WEEKLY EVENING MEETING,

Friday, January 21, 1927.

SIR ARTHUR KEITH, M.D., LL.D., F.R.S., Treasurer and Vice-President, in the Chair.

SIR WILLIAM BRAGG, K.B.E. D.Sc. F.R.S., Fullerian Professor of Chemistry.

Tyndall's Experiments on Magne-Crystallic Action.

IN 1845 Faraday made the surprising discovery that the vast majority of substances, not merely iron, nickel and cobalt, were affected by a magnet, and showed also that the action was repulsive quite as often as attractive.

Faraday's results excited the greatest interest, and were the starting point for many other researches. In fact, they paved the way for the work of Thomson and Maxwell, who came thereby to the establishment of the laws of electro-magnetism. Among the many workers who followed Faraday was Tyndall, who made certain interesting discoveries relating to the behaviour of crystals in the magnetic field.

A very lively discussion sprang up as to the mode of interpretation of the new discoveries, particularly that of the so-called "diamagnetism." On the one hand, Faraday was satisfied that he could describe them in terms of his "lines of force": the majority, including Tyndall, referred everything to the existence of poles, magnetic and diamagnetic. Tyndall's experimental work, and the consequences which he drew from it, were devoted to the support of these views. When Faraday's conceptions prevailed it became clear that Tyndall's interpretation, of his own experiments in particular, must have been incorrect. His collected account of his researches published in the well-known "Diamagnetism and Magne-crystallic Action" never became a link in the chain of argument.

The recent analysis of crystal structure by means of X-rays throws some new light on those experiments of seventy years ago. We can see more clearly where Tyndall's conclusions were in error. But at the same time—and this has seemed to be of such interest that you might like to hear of it in the evening's discourse—the experiments of Tyndall are seen to be closely related to a modern research of immense importance, that of the effect of stress on the constitution and physical properties of materials. Let me tell you the story.

On the 13th of September, 1845, Faraday made one of his most important discoveries, that of a relation between magnetism and light. He found that when plane polarised light was made to traverse a piece of his "heavy glass," a borosilicate of lead, in a direction coinciding with that of lines of magnetic force, the plane of polarisation was rotated. He had thus been successful in showing that the action of a magnet did not require the co-operation of a magnetic substance such as iron for its manifestation, but might be directly connected with a substance of a different kind, namely glass, and a different activity, namely light. In the following months he tried to find some other connection between magnetism and this glass. He floated his glass on a liquid and tried whether he could move it by a magnet, without result. But on November 4 he succeeded in his search. "The bar of heavy glass $1\frac{8}{8}$ of an inch long and * of an inch square was suspended by cocoon silk in a glass jar in principle as before and placed between the poles of the last magnet. When it was arranged and had come to rest I found I *could* affect it by the magnetic forces and give it position. Thus touching diamagnetics by magnetic curves and observing a property quite independent of light by which we may probably trace these forces into opaque and other bodies as the metals," &c. "If 1 was the natural position on making the poles magnetic the glass swung into position 2 and on to position 3. If contact was united after 2 the tendency to 3 was diminished, i.e. was less than if there was no current. If whilst swinging contact of current was continued during vibration from 1 to 2 and broken from 2 to 3, then united from 3 to 2 and broken from 2 to 1 the bar was soon sent spinning round the whole circuit" (Fig. 1).

The word "diamagnetic" is here used to denote substances through which, on his views, magnetic lines were passing. It is not yet used as an antithesis to paramagnetic. His new result obviously gave him intense pleasure, and in following it up he was so pre-occupied that he did not even go to the meeting of the Royal Society on November 20, when his paper on the "Action of Magnets on Light" was read.

We can easily repeat the experiment, using a piece of the same glass, taken from the store left by Faraday. It is not the actual piece, No. 174, as he tells us in his notes, for this cannot be found. The glass turns slowly in the magnetic field, and its motions are obviously controlled by switching the current off and on. The glass tends to set itself *across* the lines of force running from pole to pole, not *along* the lines as a piece of iron would do. And obviously the effect is very small as compared with the violent movements of iron under the same circumstances.

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* Not filled in, but from other notes we know it was half-an-inch square.

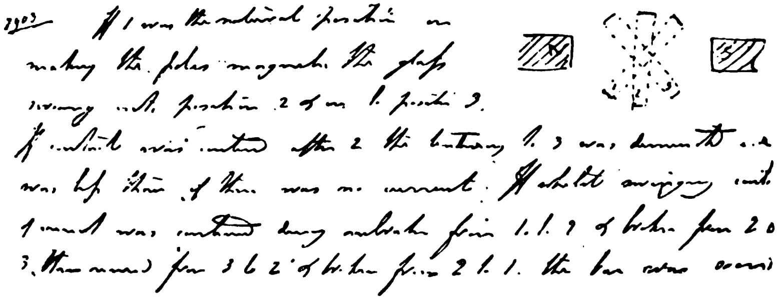

FIG. 1.—The above figure and a few lines of explanation are photographed from Faraday's original notes.

The action may be described as a repulsion of the glass by the magnet; and sometimes the early workers on the subject constructed apparatus specially designed to show the repulsive effect more obviously, and to distinguish it from a mere turning action in a magnetic field, if indeed this could be done. We can illustrate this point, and at the same time the diamagnetism of bismuth, by using a piece of apparatus constructed for Tyndall (Fig. 2).

Faraday at first suggested that the diamagnetic effect was the antithesis of the ordinary magnetic effect. A piece of iron when placed between two poles became so magnetised that a south pole was developed upon it in that part which was nearest to the north pole of the inducing magnet, and vice versa. Faraday's suggestion that the diamagnetic substance developed north and south poles where a magnetic substance would have developed south and north respectively was taken to be a satisfactory explanation. It was the constant endeavour of later experimenters to express their results in accordance with Faraday's hypothesis: even when development had reached a stage some distance ahead of that described in the original (Phil. Trans., 1846, p. 21).

Faraday was himself the first to feel doubts as to the satisfactory nature of his explanation. His early results could conveniently be described as showing an exact antithesis between two classes—where one was attracted by a magnet, the other was repelled; where one set itself in a certain direction in the magnetic field, the other avoided that direction as much as possible. It seemed proper to describe them as being in exact antithesis to each other, and the word diamagnetism was adopted as a means of expressing the experimental result.

He prepared a list of substances which showed varying degrees of response to the action of the magnetic field, and the plan of the statement illustrates his first views (Exp. Res., Vol. III., Series XXI., 2424):

Iron
Nickel
Cobalt
Manganese
Palladium
Crown glass
Platinum
Osmium
0° Air and vacuum
Arsenic
Ether
Alcohol
Gold
Water
Mercury
Flint glass
Tin
Heavy glass
Antimony
Phosphorus
Bismuth

It is to be observed that those preceding air and vacuum are to be considered above zero or magnetic, those succeeding,

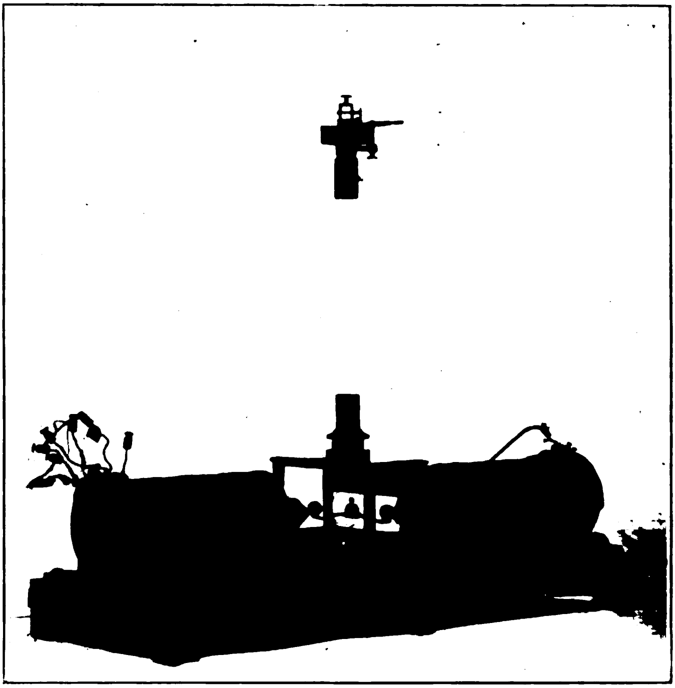

FIG. 2.—Apparatus used by Tyndall. Two balls of bismuth are placed at opposite ends of an arm swinging at the end of a suspension thread. The electro-magnets are staggered so as to combine their actions in turning the arm by means of the repulsive forces exerted on the balls. ("Diamagnetism and Magnecrystallic Action," p. 53.)

below zero or diamagnetic, which is meant to imply a true antithesis.

In December, 1845 (Exp. Res., Vol. III., Series XXI., 2429), Faraday writes:—

"Theoretically, an explanation of the movements of the diamagnetic bodies, and all the dynamic phænomena consequent upon the actions of magnets on them, might be offered in the supposition that magnetic induction caused in them a contrary state to that which it produced in magnetic matter—i.e. that if a particle of each kind of matter were placed in the magnetic field both would become magnetic, and each would have its axis parallel to the resultant of magnetic force passing through it; but the particle of magnetic matter would have its north and south poles opposite, or facing towards the contrary poles of the inducing magnet, whereas with the diamagnetic particles the reverse would be the case; and hence would result approximation in the one substance recession in the other."

But even at that time his views were not firmly established; and we may repeat an experiment of his which shows the nature of the contrary influences that were impressing him. A small glass tube filled with a weak solution of the magnetic substance, iron sulphate, sets itself axially between the magnetic poles. But if it is surrounded as it swings by a strong solution of the same substance it sets equatorially. The tube appears to be magnetic as compared to air, but diamagnetic as compared to the strong solution.

Might not, on this analogy, all substances and also air and vacuum be magnetic, reacting to the magnet in the same way, but to different degrees? And might not bismuth exhibit its peculiarities, not because it was in antithesis to iron, but merely because it was less magnetic than the air? Yet he writes as follows: "Such a view also would make mere space magnetic and precisely to the same degree as air and gases. Now though it may very well be that space, air and gases have the same general relation to magnetic force it seems to me a great additional assumption to suppose that they are all absolutely magnetic, and in the midst of a series of bodies, rather than to suppose that they are in a normal or zero state. For the present, therefore, I incline to the former view and consequently to the opinion that diamagnetics have a specific action antithetically distinct from ordinary magnetic action, and have thus presented us with a magnetic property new to our knowledge" (Exp. Res., Vol. III., Series XXI., 2440, Dec. 1845). The extract describes his first-formed opinion.

The next important step is due to Plücker:—

"In 1847 Plücker had a magnet constructed of the same size and power as that described by Faraday, his object being to investigate the influence of the fibrous constitution of plants upon their magnetic deportment; while conducting these experiments, he was induced to try whether crystalline structure exercised an influence." * The first experiment made by Plücker gave an immediate and decided reply. The investigation of the behaviour of several crystals led him to announce the following laws :—

"When any crystal whatever with an optic axis is brought between the poles of a magnet, the axis is repelled by each of the poles ; and if the crystal possesses two axes, each of these is repelled with the same force by the two poles.

"The force which causes this repulsion is independent of the magnetism or diamagnetism of the mass of the crystal ; it decreases with the distance more slowly than the magnetic influence exerted by the poles."

There is some truth in Plücker's conclusions, but much correction is necessary. Tyndall pointed out in 1850 that they broke down completely when applied to calcium and iron carbonate. These two crystals are isomorphous ; the former, Iceland spar, obeys Plücker's laws in that it sets its axis equatorially in the magnetic field, but iron carbonate sets its axis from pole to pole. Plücker had, however, done great service in calling attention to the peculiar behaviour of crystals in the magnetic field.

In the autumn of 1848 Plücker was in London. Faraday writes in his laboratory notes :—

"16 Aug. 1848. Plücker has described to me certain of his results as to the crystalline diamagnetic relation and, as I understand it, the optic axis of a crystal having *one* optic axis tends to pass into the equatorial direction, or if a crystal have two optic axes then the line between them tends to pass into the equatorial direction."

"25 Aug. 1848. To-day Plücker showed me for the first time some of his experiments.

"*First Optical Results.*

"A small rhomboid of Cal^{c} Spar was suspended by a single cocoon thread between my Elect. Magnet poles with the optic axis in a horizontal position. When the poles were very close as in the figure the diamagnetic force of the substance made it take the position shown in which the optic axis is axial to the magnet. But when the poles were opened out to distance of half or three-quarters of an inch, then the mass pointed axially and the optic axis therefore equatorially (Fig. 3). . . There is a given distance between the Mag poles (pretty close) when a certain rhomboid ‸or piece‸ of Cal^{c} spar between them is so affected that the diamagnetic and the magneto optic force is balanced at smaller distances the piece points diamagnetic and at larger distances Magneto optic. So that on increasing the distance the magneto optic force diminishes *less rapidly* than the magnetic force and on diminishing the distance it increases *less rapidly* than the magnetic force. *But increasing or diminishing the strength of the magnet produces no alteration of this place of neutral action, it only increases or diminishes the strength of the action on each side of it :—or rather the resultant of the two actions on each side of that neutral position.* So Plücker at least tells me for I did not see that proved."

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* Tyndall, "Diamagnetism and Magne-crystallic Action," p. 2.

Plücker's experiment is readily shown ; but a little care in adjustment is required. The dimensions of an equal-sided rhomb are rather too much the same in all directions : a somewhat more irregular piece is I find easier to work with. The effect is much more clearly seen with a good bismuth crystal which has been

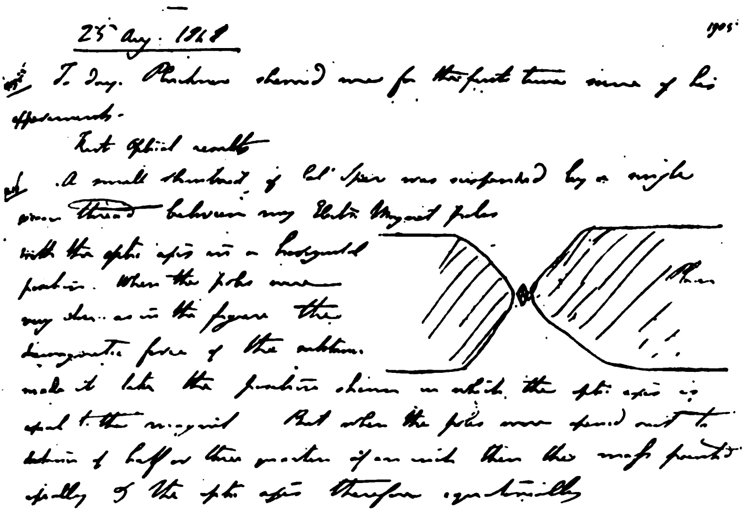

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FIG. 3.—The figure is taken from Faraday's notes, August 25, 1848.

obtained in the following way. A little bismuth was melted in a glass tube which had been drawn to a point. The tube was placed in an electric furnace, from which it was made by clockwork to emerge very slowly. The fine end of the tube came out first, and the bismuth at the point was the first to solidify into crystalline form. The rest of the metal crystallised slowly as the emergence proceeded, and under the circumstances continued the structure and orientation of the first fragment. In this way, due to Bridgeman, the mass contained large single crystals, not a mass of fine crystals as is usual when the solidification takes place rapidly.

When the crystal, which is ten times as long as it is broad, is placed in the magnetic field due to pointed poles, it sets strongly equatorially in accordance with the usual behaviour of a diamagnetic body; but when the poles are withdrawn somewhat it sets axially with equal strength.

The experiments of Plücker introduced a new effect which Faraday subsequently called "Magne-crystallic Action." It clearly deserved a name, since its manifestations added a complication to the diamagnetism which had already been observed.

The new discoveries presented so many forms when repeated with different crystals suspended in different ways and with different forms of magnetic field that the complications were not unravelled for some years. Some of the difficulties were due to the circumstances of the experiments, and had no relation to the real question. One of these incidental effects was that of attractions and repulsions due to transient currents induced in bodies already suspended for observation between the magnetic poles when the magnets were excited. As is well known the motion of a spinning block of copper is at once arrested by the action of such currents; on the other hand, a sheet of copper held near a pole is sharply repelled when the current is turned off, and if properly suspended can be set into a rapid spinning. These effects had nothing to do with diamagnetism, but they were apparently the cause of confusion on some occasions.

Another great source of difficulty was the overpowering effect of iron impurities; the diamagnetic effects are so feeble in all cases that a mere trace of iron, nickel or cobalt is sufficient to mask them. So, for instance, Plücker's experiments with antimony seem on this account to have been at variance with the true facts as proved by Faraday.*

In 1848 Faraday published a series of researches on the magne-crystallic phenomena, which cleared up some of the difficulties. But in 1850 he could still write as follows:—

"Four years ago I suggested that all the phenomena presented by diamagnetic bodies, when subjected to the forces in the magnetic field, might be accounted for by assuming that they then possessed a polarity, the same in kind as, but the reverse in direction of, that acquired by iron, nickel and ordinary magnetic bodies under the same circumstances. This view was received so favourably by Plücker, Reich and others, and above all by W. Weber, that I had great hopes that it would be confirmed; and though certain experiments of my own did not increase that hope, still my desire and expectation were in that direction. (2641.) Whether bismuth, copper, phosphorus, etc., when in the magnetic field are polar or not, is however an exceedingly important question; and very essential and great differences in the mode of action of these bodies under the one view or the other, must be conceived to exist. I found that in every endeavour to proceed by induction of experiment from that which is known in this department of science to the unknown so much uncertainty, hesitation and discomfort arose from the unsettled state of my mind on this point that I determined if possible to arrive at some experimental proof either one way or the other. This was the more important because of the conclusion in the affirmative which Weber had come to in his very philosophical paper. . . (2642.) It appeared to me that many of the results which had been supposed to indicate a polar condition were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of action; others again appeared to have their origin in induced currents. . ."

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* Tyndall, "Diamagnetism and Magne-Crystallic Action," p. 16.

Accordingly he undertook a further series of researches which in the end brought him to regard all his effects as expressible in the simple form with which we are familiar. In his "Experimental Researches" (Vol. III., Ser. XXVI., Oct. 1850, 2807), he writes:—

"When a paramagnetic conductor, as for instance a sphere of oxygen, is introduced into such a magnetic field considered previously as free from matter, it will cause a concentration of the lines of force on and through it so that the space occupied by it transmits more magnetic power than before. If, on the other hand, a sphere of diamagnetic matter be placed in a similar field it will cause a divergence or opening out of the lines in the equatorial direction and less magnetic power will be transmitted through the space it occupies than if it were away" (Fig. 4).

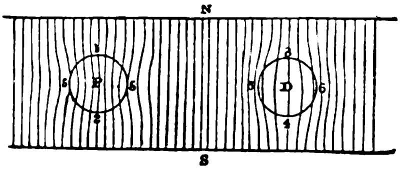

Fig. 4.—The figure is taken from Faraday's Experimental Researches, and was drawn to show his conception of the passage of lines of magnetic force through paramagnetic and diamagnetic bodies respectively.

This describes diamagnetism generally; the complication of magne-crystallic action is described with equal simplicity:—

"(2837). If the idea of conduction be applied to these magne-crystallic bodies, it would seem to satisfy all that requires explanation in their special results. A magne-crystallic substance would then be one which in the crystallised state could conduct onwards, or permit the exertion of the magnetic force with more facility in one direction than another; and that direction would be the magne-crystallic axis. Hence, when in the magnetic field, the magne-crystallic axis would be urged into a position coincident with the magnetic axis by a force correspondent to that difference, just as if two bodies were taken, when the one with the greater conducting power displaces that which is weaker."

It is only a uniaxial crystal of course which possesses a single magne-crystallic axis; it is the axis of a certain spheroid. The facility of conduction in different direction in a biaxial crystal requires an ellipsoid for its representation.

This way of stating the rules allows us to see at once the principle of the experiments which Plücker showed to Faraday, and which the latter so greatly extended. When the magnetic poles were close, the crystal occupied a part of the field where the lines of force were very divergent. Under such circumstances the orientation of the crystal would be determined by the general tendency for diamagnetic bodies to move from the stronger to the weaker parts of the field, and the crystal set its longer dimension perpendicular to the field; the optic axis was then parallel to the lines of force. But when the magnetic poles were separated the crystal covered a part of the field in which there was little divergence; the magne-crystallic action then took charge, and the crystal set itself so that the direction of worst conduction of the lines, i.e. the optic axis, was at right angles to the lines.

A few simple experiments will serve as further illustration of these rules. We take a crystal of sulphate of iron which has the form of a thin plate: the flat sides are cleavage planes and the "conducting power" for Faraday's lines is far greater across the plate than along the large faces. In a uniform field the crystal plate sets equatorially therefore, and even when allowed to move up to one of the poles keeps its face normal to the lines. A thin plate of iron would stand on edge on the pole; but the magne-crystallic action of this paramagnetic crystal is exceedingly strong.

A bismuth crystal so suspended that its axis (it is a uniaxial crystal) is vertical has no magne-crystallic action. Its motions are governed by the general tendency of its mass to move from the stronger to the weaker parts of the field; in a uniform field it has no appreciable tendency to set itself in any particular direction. But when the crystal is hung so that the axis is horizontal, that axis tends strongly to set itself along the lines of force, as we saw before.

Naphthalene is a monoclinic crystal. Its magne-crystallic properties are represented by an ellipsoid, one axis of which coincides with the single axis of symmetry. The cleavage is very perfect and the crystals take the form of thin plates parallel to the cleavage planes. The axis is also parallel to the cleavage planes and if the crystal is suspended so that this axis is horizontal and the cleavage planes vertical, the axis and the planes move into the equatorial position, thus fulfilling the requirements of symmetry. But when the crystal is hung, in a uniform field, so that the axis is vertical the cleavage planes set themselves at a certain angle to the field. One of the axes of the magnetic ellipsoid then lies along the lines, another across them; the third is vertical. There is no obvious relation between the cleavage plane and the first two axes. Finke* has shown that this may be said of various crystals examined by him. If now the crystal be suspended from the other end of the axis, its cleavage planes will make the same angle with the field, but on the opposite side of the medial line (Fig. 5). Faraday describes results of this kind which he obtained with a paramagnetic crystal of sulphate of iron (Exp. Res., Vol. III., Series XXI., 2634–7). Naphthalene is diamagnetic; like many other organic crystals it shows the magne-crystallic effect very strongly.

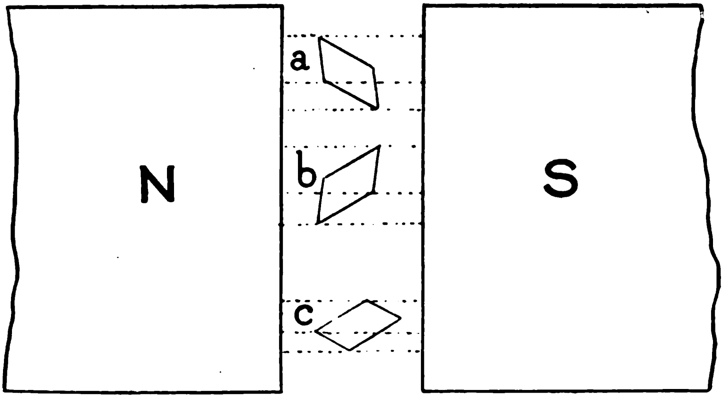

Fig. 5.—In this figure the positions marked *a* and *b* are positions of equilibrium of the naphthalene crystal in the magnetic field. The axis of symmetry is vertical and perpendicular to the plane of the paper. The cleavage plane is also vertical, and its intersection with the plane of the paper is the longer side of the rhomboid. The form of the crystal shown in the figure is not a natural form because the shorter side of the rhomboid is drawn parallel to one face of the cell of the crystal lattice, which face does not usually occur on the crystal. It is so drawn in order to show the relation between magnetic lines and the lattice.

If the crystal is hung from one end of the *b* axis the position *a* is assumed, and if from the other the position *b*. The position *c* is impossible.

These experiments will serve to show the great variety of effects that may be observed. All of them are, however, easily correlated by Faraday's conception of lines of force. Let us remember that there are several variables and give due importance to each. The first of these we call diamagnetism, implying that the lines pass through the substance in question less easily than through the air or a vacuum. The second is called magne-crystallic action, in reference to the fact that in a crystal the lines pass more easily in one direction than another. A third variable is the crystal shape, which may also affect the set in the magnetic field when the latter is divergent. A fourth is the amount of divergence of the field which, in a uniform field, falls to zero. All these influences have to be taken into account after experimental disturbances have been allowed for.

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* Annalen der Physik, xxxi., 149, (1910).

The more divergent the field the more does the simple diamagnetic effect assert itself, and any magne-crystallic action which would tend to make the specimen set a crystal axis or axes at some particular inclination to the direction of the field is overpowered.

On the other hand, magne-crystallic action usually takes charge in a truly uniform field. For the sake of brevity and an easier explanation it may be well to call attention to a fact which was not fully appreciated by all the first experimenters, but was clearly set out by Sir William Thomson (Lord Kelvin) in 1885. A diamagnetic bar, apart from magne-crystallic action, tends to set itself *along* the lines of force in a *uniform* field, just as a paramagnetic bar. Imagine the bar to be made up gradually of a collection of cubes, placed successively one after the other in the magnetic field. The effect of the presence of the first cube is, as we should say in the language of Faraday, to spread out the lines of force on their way through the cube, and to crowd them together on either side of it. A second diamagnetic cube will, if free to move, go to that part of the field where the lines are least crowded; it will therefore avoid setting itself beside the first cube and prefer to place itself in front or behind it. A third will continue the same process, and in the end the cubes will form a bar pointing along the lines. In the case of a substance such as iron there is a double converse. The lines are most dense just in front and just behind the first cube; and a second cube will place itself in one of those positions because a magnetic substance seeks for the strongest part of the field. Again, therefore, the bar grows along the line as, in this case, we know from experience (Fig. 6).

It is certain that no one has ever seen the first of these effects because it must be so minute and difficult to separate from others. We may safely infer it, as Thomson pointed out, because our theories of the electromagnetic field have been abundantly justified by other means. The susceptibility of bismuth, by far the most diamagnetic substance, is only about 10^{-6}; in other words, the strength of the field on one side of a bismuth cube of 1 cm. side would only be about a thousandth part of one per cent. greater than at the front or back of the cube. Near a pointed pole the strength of the field might easily vary by fifty per cent. in a centimetre. It is easy to see how feeble is the force tending to arrange the supposed cubes parallel to the lines of a uniform field as compared with the forces acting on a bar placed near the pole.

The analogous effect in electrostatics can however be realised. When two plates are immersed in oil and maintained at a large difference of potential, an elongated rod of glass hung from a fibre, sets itself along the lines of force; to make sure that the effect is true the rod must be free from any conductivity due to its own substance or a water film. This corresponds to the setting of a magnetic body in a uniform field. When bubbles of air are allowed to rise through the oil they are drawn out along the lines of force,* which

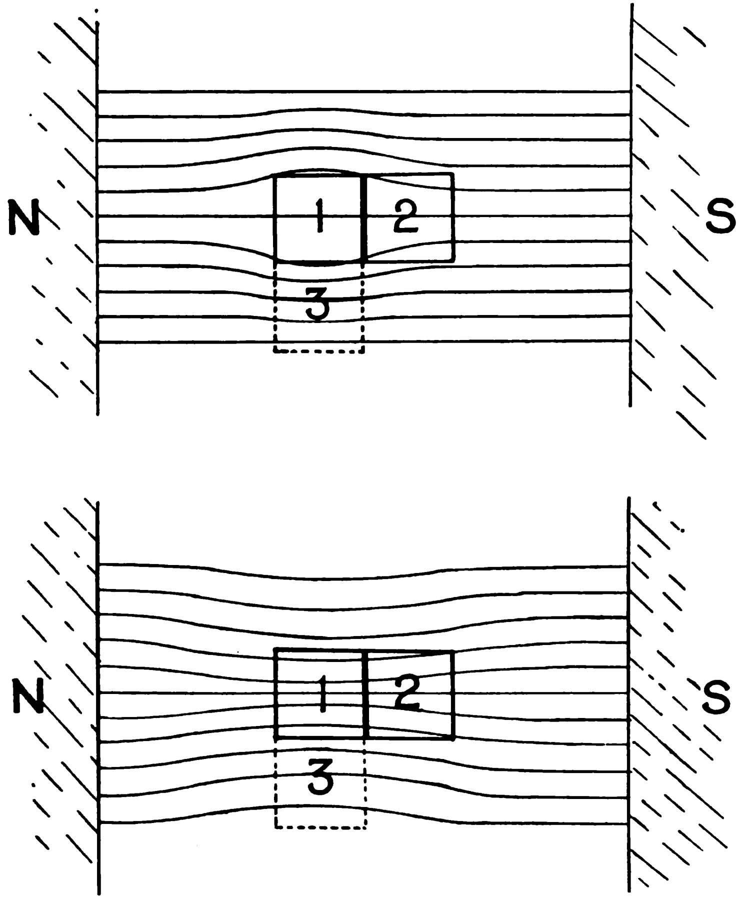

Fig. 6.—Dia- and paramagnetic substances in a magnetic field. In both cases Position 2 is preferred to Position 3.

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* I am so informed by Capt. Dunsheath of the Henley Telegraph Works Co.

effect, since the inductivity of air is less than that of oil, represents the setting of a diamagnetic body along lines of magnetic force.

When a diamagnetic substance sets itself across the lines of a magnetic field, and no magne-crystallic action is at work, it is because the field is not really uniform. It is perhaps a little confusing when it is said, as is sometimes the case, that diamagnetic and paramagnetic substances are the antitheses of one another in that one kind sets itself across the field and the other along it. This is only true of a field which is non-uniform. Indeed, it may be said that the use of the word antithesis is incorrect in any case. There would be a true antithesis if one substance could be defined by its pointing along the lines of force in one direction while another pointed in exactly the opposite direction ; there is no true antithesis between pointing along the lines and pointing across them. It seems to me, though I say it with diffidence, that this difficulty was stirring in Faraday's mind, and was the true cause of the uneasiness of which he spoke in a quotation given above, and of his aversion to the description of diamagnetism and paramagnetism as being the antitheses of one another.

Faraday, as I have said, when this thorough examination of the facts had led him to frame a hypothesis which would link them together, based his interpretation on the existence of lines of force, and found himself able to place both his own results and those of others in their place within a self-contained system.

Kelvin placed this hypothesis in mathematical form, thus completing the treatment of the subject of magnetism by Poisson : the latter had left out of his consideration the consequence of magnetic susceptibility being different in different directions, not because he overlooked the possibility of such an effect, but because no case of its occurrence was known to him.

But Faraday's views were not accepted by other experimenters on the same subject, and in particular by Tyndall. The idea of polarity was not to be given up easily, and innumerable experiments were made to show that a "diamagnet" had poles like a magnet, but in the opposite sense. A bar of bismuth would develop north and south poles, when under similar circumstances a bar of iron would develop south and north. And, of course, when the facts are prepared for mathematical treatment they can be expressed in this way. It is generally convenient and justifiable to represent a magnet by two poles because the form of the field at any reasonable distance is satisfactorily represented thereby, though in the immediate neighbourhood the lines of a real magnet do not run like those of the theoretical bipole. Within the body of the magnet the lines run from the south pole to the north, continuing and completing their course outside, so that every line is a closed circuit ; but all lines near a bipole run from the north pole to the south pole. So also in the magnetic shell, which is in the mathematical treatment the exact analogue of the electrical condenser, the bulk of the lines run from one plate to the other across the narrow space between the two plates; comparatively few run from the outside of one plate, through surrounding space, to the back of the other. In the condenser the internal field is the more important, the external being looked on as a correction. In the magnetic shell the reverse is the case; the outside field is that which is considered because it represents more and more closely, as the plates are brought closer together, the field due to a current circulating about the contour of the condenser. The theoretical charges on the plates have to be made larger and larger as the plates are brought together so that the strength of the outside field may remain the same.

Now, if a piece of bismuth is placed along the lines of a magnetic field, the lines avoid the piece to some extent, though, as already explained, the avoidance is extremely small. If we take the bismuth away and replace it by a feeble bipole consisting of south pole magnetism, of proper amount, where the lines come out of the bismuth and a corresponding amount of north pole magnetism where they go in, the whole arrangement being made in a vacuum, or, permissibly, in the air, we get in this artificial way an external field resembling that which exists when the bismuth is in place (Fig. 7). It can be said that polarity is developed in the bismuth in a sense opposite to that which is to be found in iron under the same circumstances. If the effect is to be represented, for the convenience of treatment, as due to the presence of a bipole, then the sense of the bipole in the case of bismuth is opposite to the sense in the case of iron. The old argument therefore was not between two hypotheses but two languages in terms of which the facts were to be described. Such an antagonism, once believed in and debated, could be, and was, the cause of an immense variety of experiments devised to justify one side or the other. But Faraday felt that his way of putting the facts was more fruitful in suggestion of further experiments, and more convenient as a foundation for theoretical development. He has been abundantly justified.

We now come to the part which Tyndall played in a debate which was conducted on both sides in such an able, and, it is pleasant to observe, in such a friendly way. In the first place, Tyndall and Knoblauch published in 1850 an account of experiments which they had made. They showed that Plücker's first views, those which included the repulsion of the optic axis of a uniaxial crystal by the poles of the magnet were, as already stated, incorrect in many cases, and they substituted an amended set of rules in the following terms:—

"If the arrangement of the component molecules of any crystal be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that chosen by the respective forces for the exhibition of their energy. If the mass be magnetic this line will set axial, if diamagnetic equatorial."

The key-word is "proximity." This condensed statement, of course, requires explanation. Tyndall supplies it in full in his book on "Diamagnetism and Magne-crystallic Action." A brief summary will be sufficient for our present purpose. In the first place, the observations made by himself and his partner on the behaviour of crystals in the magnetic field convinced them that the plane of cleavage determined in a number of cases the position which the suspended crystal would take. Magnesium sulphate, zinc sulphate, saltpetre, and topaz were diamagnetic substances, and their cleavage planes, the crystals being so suspended that these planes were vertical, always set themselves equatorially, i.e. at right angles to the field. On the other hand,

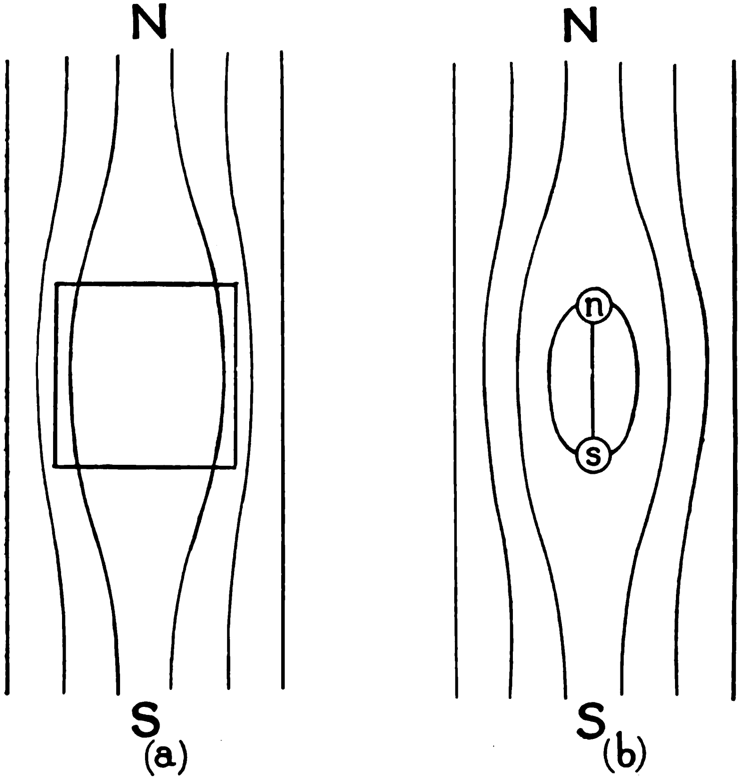

Fig. 7.—For explanation see the text.

nickel sulphate, scapolite and beryl, which were magnetic crystals, under the same circumstances set their cleavage planes parallel to the field. The connection between these results and the statement quoted above lies in this, that the molecules in a crystal were supposed to be in greater proximity along a cleavage plane than in any other direction. Let us take bismuth as an example. It is diamagnetic and sets its cleavage plane equatorially in accordance with Tyndall's rule. Its structure has now been determined by X-ray analysis, so that we can see what meaning can be attached to the claim for proximity in the plane of cleavage. The bismuth structure can be looked on, approximately, as a slightly distorted cube ; one of the cube's diagonals has been a little stretched, while the other three have been left unchanged. The crystal is then uniaxial ; the axis is the stretched diagonal ; the principal cleavage plane is perpendicular to the axis. The spacing of the planes parallel to the cleavage is larger than that of any other set of planes in the crystal, and these consequently contain more molecules to the unit area than any other planes. Tyndall would have said that in those planes there was a maximum proximity between the molecules.

The X-ray analysis of other crystals often shows the cleavage plane to have the largest spacing and therefore the closest degree of packing. This means that the points of the crystal lattice are closest together in that plane, but it does not mean that the atoms or molecules are nearer together in that plane than in any other. Nothing can be said about that until the actual distribution of the atoms in the unit cell has been determined. It would be much safer to say that the existence of a cleavage plane implies a certain *looseness of packing across* the crystal planes, which are parallel to the cleavage. This would imply a greater tightness in other directions, but not necessarily a greater proximity. It is only at first glance that the latter term seems to have a clear meaning. But we must let it stand in order to realise the argument as it presented itself to the authors of the statement quoted.

It happens that in the case of bismuth we do actually find a closer bonding between the atoms in the cleavage plane than in any other ; but this is peculiar to the structure of bismuth, and has no relation to the supposed close proximity of *molecules* in the cleavage plane.

Now we come to the essential point of the argument. It is supposed that proximity offers magnetism or diamagnetism, whichever it may be, the opportunity to "exhibit its greatest energy." We are to remember that the hypothesis on which we are working expresses itself in terms of poles, and that a magnet attracts a piece of iron by inducing poles in it, which poles then react with the poles of the magnet. When a piece of iron is allowed to attach itself to a magnet, the poles induced in it are much stronger than if the magnet and the iron are separated by a little distance. If a second piece of iron is brought near the first, every increase in its proximity increases the strength of the poles which are developed in this piece by the influence both of the original magnet and of the first piece of iron. A simple experiment will serve as an illustration (Fig. 8):—The nail hangs in the first case, and not in the second, because the close proximity of the iron blocks increases the strength of the poles in all of them. In the second case, the nail will not hang, although the

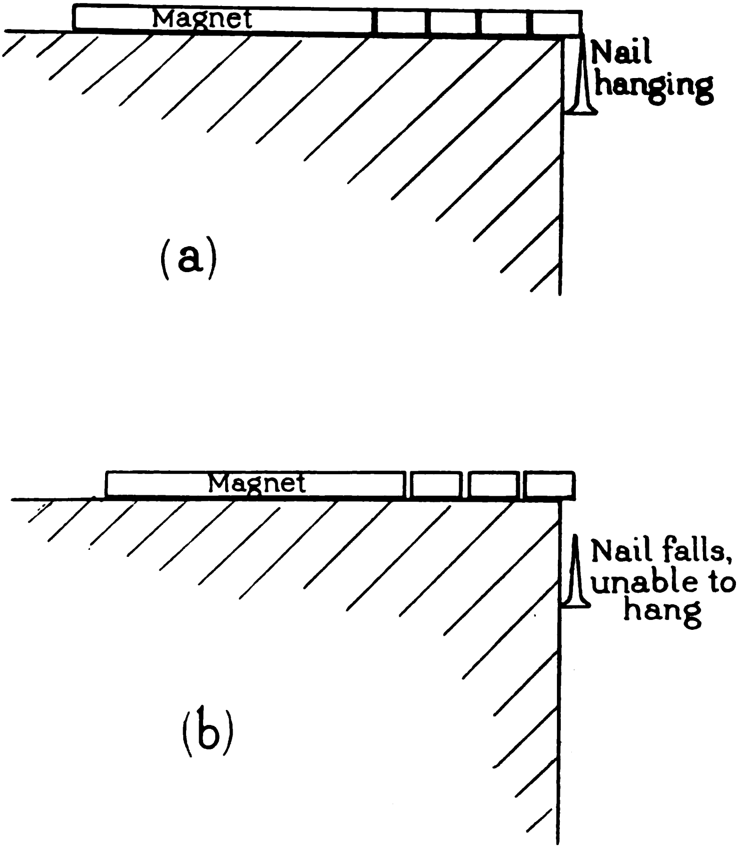

Fig. 8.—When pieces of iron are all in contact with one another and the end one with the magnet, the nail can hang as shown in (*a*). But when, as in (*b*), the iron blocks are somewhat separated from the magnet the nail falls.

magnet is actually closer to it. The benefit of mutual "proximity" of the separate pieces of iron is obvious. An equally simple explanation can be given in terms of lines of force, but we are using the alternative language.

Chains of iron fragments form readily between magnetic poles of opposite nature. A rod of iron "transmits the magnetic force," and generally acts more efficiently than a set of iron fragments which are not allowed to get into close proximity with each other. Tyndall sticks short lengths of iron wire through disc-shaped pieces of apple, and shows that the disc set itself at right angles to the field, the bits of wire therefore lying parallel thereto. In each bit are many molecules of iron in close proximity, and the fact is more effective in directing the apple than the existence of a number of bits scattered over the disc without being in "proximity" to each other.

It is now argued by Tyndall that if the magnetic influence of a magnet is extended by means of proximity, the diamagnetic influence must be extended in the same way. If the close proximity of iron fragments will help them to set with greater firmness in the direction joining opposite poles, then the closer proximity of bismuth fragments should cause them to set with greater firmness across them. In this way Tyndall interpreted the rule, which he believed he had established, that the cleavage planes of magnetic crystals tended to set axially, and those of diamagnetic crystals equatorially.

It is interesting to observe that Tyndall was attempting to supply both a rule for the setting of crystals and an explanation of the rule in terms of structure. Faraday stopped short when he had supplied a picture of the distribution of his magnetic lines, or, as we should now say, a map of the distribution of energy in the magnetic field.

As I have already pointed out there is no clear meaning to the term "greater proximity in the cleavage plane." Moreover, if conclusions were to be drawn from analogy with phenomena on a larger scale they would run contrary to the intended argument; for on that scale at least a line of diamagnetic masses tends to set itself axially, not equatorially. Moreover, a piece of bismuth makes an extremely minute alteration in the disposition of the lines of force, for which reason it is a very poor detector of the existence of the lines in comparison with iron; and the change, since it is so small, can indeed be detected by a piece of iron in the form of a magnet, but certainly not by another piece of bismuth, no matter how close they are together.

In the case of a uniaxial crystal a principal cleavage must from symmetry considerations be related to the axis and, if it is unique, must be perpendicular thereto. When there is more than one cleavage, the cleavage planes must be symmetrically disposed about the axis, as in the case of Iceland spar. It is not therefore surprising that in the former case the cleavage plane should place itself exactly, either equatorially or axially, and that in the latter case a plane perpendicular to the axis might be looked on as a resultant of cleavage planes and therefore set itself equatorially. But it does seem remarkable that, as Tyndall pointed out, the substitution of iron for calcium in Iceland spar, to form the isomorphous iron carbonate, should turn the structure round through 90° in the magnetic field; especially if we assign diamagnetism and paramagnetism to different causes. It may be there are other cases of the same change; and any rule of this kind must clearly be of importance.

We now come to another set of experiments, very interesting and important, which were used by Tyndall in the defence of the "polarity" position. The method of these experiments was suggested by an accident. When working in Berlin with a fine magnet placed at his disposal by Magnus he was observing the action of the magnet on a bismuth cube which was so shaped that two opposite faces were perpendicular to the optic axis and parallel to cleavage planes. When the current was switched on the magnetic poles rushed together because the separate parts of the magnet had not been properly bolted down. The bismuth cube was crushed to some extent. Working conditions having been restored, it was found that the bismuth set itself at right angles to its former position. The line of pressure, which of course had been parallel to the field, was now perpendicular to it. Tyndall now argued that the particles of bismuth had been brought into greater proximity by the pressure, and that the setting of this line of great proximity was in accordance with the rule given by himself and Knoblauch. So began an extended series of researches on the effects of pressure which are fully described in his book. As an example let us take the following:—

"A quantity of bismuth was ground to dust in an agate mortar, gum-water was added, and the mass was kneeded to a stiff paste. This was placed between two glasses and pressed together; from the mass when dried two cubes were taken, the line of compression being perpendicular to two of the faces of each cube and parallel to the other four. Suspended by a silk fibre in the magnetic field, upon closing the circuit the line of compression turned strongly into the equatorial position . . ."

When carbonate of iron was used the line of pressure set axially.

Such an experiment is very striking, whatever its explanation may be. Tyndall argued that he had by compression increased the proximity along the line of pressure, but it is difficult to see how this can be. If a number of particles of one kind are distributed with complete irregularity in a paste medium which is then subjected to pressure in one direction, the alteration in form of the paste block will not alter the law of distribution of the particles. In any case, as we have already shown, proximity does not produce any observable effects.

It is surely natural to suggest that the particles acquired some orientation from the pressure, which might well happen, if they possessed shapes which were related in some particular way to their structures. Thomson immediately pointed this out to Tyndall, who replied that if that were the case, the bismuth fragments being naturally in the form of flakes parallel to the cleavage planes, the line of pressure ought therefore to set itself axially, whereas it actually set equatorially. This was certainly a good reply. Perhaps the counter argument is that the crystal fragments have not actually been shown to set in this way. Miss Knaggs has made an X-ray measurement of the set of the fragments in one specimen of squeezed dough containing bismuth particles, and has found that the cleavage planes are not closely co-planar with the surface. as they must be if Tyndall's argument is to be good. Though this is a single example, it looks as if a way of escaping the difficulty was to be found.

As I have said, Tyndall's reply to Thomson was good, but, to use his own words, though it formed "a strong presumptive argument it was not yet convincing." He strengthened his case greatly by a further experiment. Comparing the repulsion exerted by a magnet on a natural crystal of bismuth with that exerted on a mass of compressed powder in dough he found the latter greater than the former. He had cut the crystal into the form of a cube and placed it on one arm of a torsion balance so that the cleavage plane was perpendicular to the magnetic field, and the repulsive force as great as it could possibly be. The dough had been pressed into a cube of the same size and placed with its line of pressure at right angles to the field. Tyndall argued that there must be a direct effect of pressure, since it had done more than all that the natural phenomena could do.

Now it is clear that if the orientation of a bismuth crystal in a uniform magnetic field, i.e. the magne-crystallic action, is due to the arrangement of the atoms and molecules in the crystal structure, the perfect crystal ought to show the effect more perfectly than the fragments distributed through the dough, however perfectly the latter may be arranged. But Miss Knaggs has made an X-ray photograph from the face of a natural "crystal." The specimen was clipped out of a mass of crystals left in a crucible, and must have resembled that which Tyndall used. The photograph showed at once that the specimen was a compound of more than one crystal, and that different orientations were present. Cleavage planes, and also others which in a single crystal would make large angles with the cleavage planes, were nearly parallel to the face under test. It is possible therefore that there was really more of the effective orientation in the pressed specimen than in the natural piece. A photograph of the single crystal made by Bridgman's method taken in the region of the cleavage plane gave a much cleaner picture.

A piece of bismuth can be looked on as an aggregate of crystals. There may be but one perfect crystal, or there may be a number, small or large, of smaller crystals, each perfect. If proximity were increased by pressure, the change in proximity would have to occur in respect to the mutual distances in either of the separate crystals, or of the atoms and molecules in the single crystal. The X-ray analysis shows that the latter alternative is impossible because from many tests recently carried out in respect to metal structure we learn that no permanent change in the crystalline lattice is occasioned by stress. The former alternative is also ruled out, because as Faraday pointed out* bismuth is actually of a lower density after compression than it was before; the pressure having of course been removed. Apparently the breaking up of the specimen increases the extent of the cavities.

Tyndall made many paste models of crystals, mixing powders of bismuth, carbonate of iron, or other active substances with flour and water, or gum. He pressed the mass by different amounts in different directions and then cut it to shape; in this way he imitated the magne-crystallic action in detail. At one time, in order to meet the objection that he was merely rearranging the small crystals in his paste and conglomerates, he took some white wax "concerning whose amorphism there can be but little doubt." The substance is diamagnetic. A little cylinder of the wax suspended in the magnetic field set with its axis equatorial. It was then placed between two stout pieces of glass and squeezed as thin as a sixpence; suspended from its edge the plate thus formed set so that its length, which coincided with the axis of the previous cylinder, was axial and its shortest dimension equatorial. But we know now that wax is anything but amorphous; its crystalline structure has not only been observed but accurately measured; and we know also that pressure arranges the orientation of the crystals.

He obtained the same result with a piece of bread, and we may repeat the experiment. A small piece of the crumb is squeezed between two glass plates, and the edges of the irregular mass are trimmed off, so as to leave a thin disc. When this is suspended so that its plane is vertical it sets equatorially if the poles are close together and the field is very divergent. It is therefore diamagnetic. But when the bread is moved from the space between the poles to a more uniform part of the field, the plane of the disc turns through a right angle and sets itself parallel to the lines of force. It is quaint to observe how the bread, as it is moved up the poles, sets itself to pass neatly through the narrow gate and take up a parallel position on the further side. This is due to magne-crystallic action, so that the bread contains crystals, a fact easily verified by X-ray methods.

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* His reference was to Gmelin's "Handbook of Chemistry," iv. 428.

The long series of interesting and ingenious experiments which Tyndall made to show that pressure produced proximity, and proximity produced the equivalent of magne-crystallic action, must be held to have failed in their original purpose. But they will doubtless be put to a different use. They are related to a subject of immense importance in these days—namely, the effects of pressure, and tension and mechanical treatment generally upon the state of a material and upon its physical properties. The consideration of such questions is fundamental to metallurgy and to other great industries. The microscope has for many years been employed for the purpose, and the new methods of X-ray analysis are already being put into service. It may well repay us to consider Tyndall's experiments in a new light; and to examine the actual nature of those rearrangements which produced such remarkable changes in magnetic reactions. Tyndall himself discussed the effects of pressure in producing planes of possible cleavage, and was one of the pioneers in showing how such planes, occurring in the earth's crust, were not always to be interpreted as the result of sedimentary deposition, but rather of pressure, which might, if it were exerted more or less along the deposition planes, produce cleavages across the latter. He extended the principle to account for stratification in rolled materials, even in biscuits and pastry.

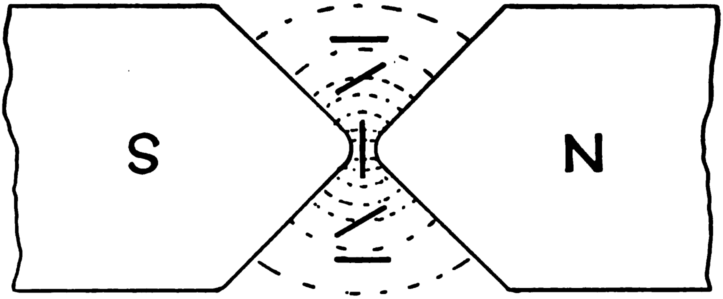

FIG. 9.—The black lines show different positions of a thin wafer of pressed bread hung by a single fibre. In the outer parts of the magnetic field it sets more or less along the lines, but as it is brought up to the more intense parts where there is great divergence of the lines it turns so as to set itself at right angles to the field.

Faraday's use of lines of force did not, in reality, demand so much framing of hypothesis as Tyndall's polarity. It is to be observed that, as Faraday pointed out, they had no differences about facts, merely about methods of description, which methods, however, were of different value as suggesting development. To Faraday's conceptions have been added theories of magnetism and diamagnetism based on the existence of resistanceless molecular circuits as imagined by Ampère and Weber, or on revolving electrons as explained by Langevin. In the most recent times the quantum theories have again modified our ideas.

The crude hypothesis of the molecular circuit leads simply to a useful point of view of the difference between paramagnetism and diamagnetism, and the most modern discussions though they differ greatly in appearance leave that point of view almost untouched. If any of Faraday's lines of force thread a circuit which has no electrical resistance, that number can never be changed. If, therefore, a substance be brought into a magnetic field, the molecular circuits in the atoms of the substance act like obstructions to the lines, and the total obstruction, of which the negative magnetic susceptibility is a measure, is proportional to the sum of the areas of all these circuits, as projected on a plane perpendicular to the lines. It is of no consequence whatever whether there are already currents in those circuits; unless indeed the circuits are movable and can alter their set towards the imposed field. Thus the diamagnetism is unaffected by the existence of molecular magnetic fields; or by any changes in them, so long as the total of the projected areas of the circuits is unchanged. This result does not hold if circuits approach each other so closely that they offer less obstruction to the lines than if they were more separated. Two resistanceless circuits running closely parallel to each other offer little more opposition to the passage of lines than either circuit alone. We should imagine that such changes in the relative position of circuits would only occur under strenuous circumstances such as possibly those of crystallisation. It is known that diamagnetic susceptibility may vary very slightly; for instance, Oxley has shown that crystallisation sometimes brings about small but definite alterations. As has often been pointed out, this simple theory makes diamagnetism a property of all substances, which can be affected, even overwhelmed, when the circuits already contain currents, and therefore can be orientated afresh by the magnetic field.

Let me say in conclusion that although recently acquired knowledge of the structure of materials leads us to reconsider Tyndall's experimental results, we are still far from the full explanation of the connection between structure and magnetism, and of the influence of the latter upon physical properties.

[W. H. B.]

WEEKLY EVENING MEETING,

Friday, January 28, 1927.

J. MITCHELL BRUCE, C.V.O. M.D. F.R.C.P., Vice-President,
in the Chair.

EDWARD P. CATHCART, C.B.E. M.D. D.Sc. F.R.S.,
Gardiner Professor of Chemical Physiology, University of Glasgow.

The Physique of Women Employed in Industry.

IN spite of the large amount of work which has been done on the anthropometry of man, but little attention has been given to the determination of the physical measurements of women. This is very clearly seen, for instance, if the classical report of the Anthropometric Committee of the British Association be examined. The insistance on the measurement of the male is perhaps even more strikingly demonstrated in the tabular matter of Baldwin's Physical Growth and School Progress Report. This shows that, although there is ample material available for males of all ages, and a fair amount of data for female children, there is a real deficiency of data concerning women. Miss Lucy Cripps in her report gives data for a number of women, but these are almost exclusively of the student class.

The question might well be asked, Of what moment is it that the information is not available? Quite apart from the fact that it would be a matter of some general interest to have a body of data on the physical condition of women available, this information would be almost certainly essential if any legislative action in the form of further welfare work on behalf of women, particularly with regard to protection from overloading, was contemplated. As a matter of fact the present inquiry was begun in response to a request from the Secretary of State for Home Affairs to the Medical Research Council for information which would guide him in laying down, if considered necessary, the optimum load to be carried by women and female young persons in factories. I was requested by the Council to undertake the inquiry.

As regards the mode of attack it is obvious that there are, at least, two promising methods: one, to have a series of weighings of loads actually carried by women in factories; the other, to determine, by experimental means if possible, the optimum load. In a previous piece of research work undertaken in conjunction with Captains Richardson and Campbell for the Army Hygiene Advisory Committee on the optimum load to be carried by the soldier, we had found that the load could be stated in general terms as a percentage of the body weight. As it was not difficult to obtain the mean weight of the soldier, it was therefore easy to state the actual weight, which should not be exceeded, for the average soldier. The attempt to apply these methods to women shows up at once the poverty of physical data. Even although the actual loads were determined by a very large series of weighings, and although by experimental methods it could be shown that a load weighing so many pounds was the optimal load, if these values are to be converted into a form suitable for insertion in a parliamentary bill, the loads would require to be related, not to the limited number of individuals measured or to the few laboratory subjects, but to the average woman engaged in industrial work.

Obviously if we were to carry out the new investigation along the same lines as were employed in the Army investigations—and our previous results appeared to justify this course—we had to obtain anthropometric information about the women engaged in industrial work.

We accordingly attacked the problem along the following lines:—

1. The determination of the physical characters of the average women engaged in industry.
2. The determination in the laboratory of the optimal load.
3. The determination of loads actually carried in the course of ordinary work by women.

The Chief Inspector of Factories and the late Senior Medical Inspector of Factories detailed Dr. S. Overton, Woman Medical Inspector of Factories, to carry out a series of special measurements of loads actually carried in factories. Dr. Overton was also asked to obtain the body weight of the woman carrying the load at the time the measurement of her load was made. Dr. Overtou obtained information from 417 women and female young persons. Her report was of great practical value, giving details, as it did, of conditions within actual factories.

As regards our side of the work, Dr. Catherine Blair and Miss Enid Weatherhead, B.Sc., obtained a series of measurements from

3076 factory women drawn in samples from areas in Glasgow, Lancashire, the Midlands and London. In addition to the examination of the women actually engaged in active factory life, through the courtesy of the Ministry of Labour they were allowed access to the Labour Exchanges. They determined the physical condition of 413 women who had previously engaged in factory work, but who had been unemployed for a year or longer. Also in order to obtain a control group of another class, through the courtesy of the Director of the Glasgow Provincial College for the Training of Teachers, they were allowed to examine 460 of the women students.

Miss Elizabeth Bedale, M.A., and Dr. Katherine Macleod carried out the laboratory series of experiments in which the physiological cost, to the organism, of the carriage of the various loads was determined from the respiratory exchange.

As my time is limited I shall confine my remarks this evening to the determination of the physical standards of the women examined. We obtained in addition to certain other information, which will be found in the full report, the age, weight, height, lumbar pull, grip and crush of the various subjects. We selected the three strength tests mentioned because we considered they would give a fairly comprehensive picture of the general muscular strength of the subjects examined. The *lumbar pull* was carried out (Fig. 1A) by the subject fixing the special dynamometer to the ground by standing on a block of wood to which it was attached, and pulling on the handle which was adjusted specially to her height. As the machine was a self-registering one there was no need to keep up tension until the reading was made. This pull not only tested the muscles of the back and the legs, but also those of the shoulders. The *hand grip* was tested (Fig. 1B), using the Smedley type of hand dynamometer with adjustable hand grip. We found it most essential that the finger piece should be a variable one, as this allowed all subjects to exert their most favourable grip. The *crush*: in the performance of both pull and grip many of the subjects utilise groups of muscles which are much used for carrying out similar operations in the course of their day's work, and hence the values obtained might be expected to vary with occupation. We introduced the crush test in the belief that, as the muscles involved were not commonly used in a continuous manner in industrial processes, it would give a better and more uniform test of the general muscular capacity of a wide variety of subjects. In carrying out (Fig. 1C) this test the arms were raised to shoulder level and the dynamometer held between the hands, clear of the chest wall, and force exerted by pressing inwards with the palms of the two hands.

We attempted to get as wide a survey of the industrial world as possible, selecting trades both heavy and light. The following table gives a list of the factories visited :—

TABLE I.

Trade	District	Number of Firms
Printing	Glasgow	1
Engineering—		
Nut and Bolt Works	,,	1
Nut and Bolt Works	Midlands	1
Chain Works	,,	1
Hollow Ware Works	,,	1
Pen Works	,,	1
Telephone Works	,,	1
Textile	Glasgow	1
	Lancashire	2
Warehouse	Glasgow	2
Bakery	,,	1
Clothing	,,	1
Chemical	,,	1
Laundry	,,	1
	London	1
Shoes	Midlands	1
Pottery	,,	2
Bricks	,,	1
Confectionery	Glasgow	1
	London	2
Soap	,,	1
Bottling	,,	1

The heaviest work noted was that carried out in the chemical works (Glasgow) and the brick works (Midlands). Next to these industries come the nut and bolt, chain and hollow ware works, bottling, laundries, and some of the work done in the Potteries. As regards the remainder, except for a few operations, the work done was more or less sedentary. The work in many factories has degenerated into "machine minding," but although so much of it is now performed automatically, it must not be inferred that it has abolished skill. On the contrary it has only altered the type of skill. High-speed machinery in many occupations demands both manual skill and alertness of mind.

By far the most interesting works from our point of view were the two already referred to, as those in which the heaviest work was done, viz. the chemical works and the brick works. In the chemical works the girls employed were literally remarkable for their physique and the grace of their carriage. They all worked bare-footed and carried out their various operations with great skill and ease. The astonishing thing was that the majority of these perfect young women—no girl was employed under the age of 16—were born and bred in one of the worst districts in Glasgow. We were told that the mothers and grandmothers of many of the women

A

B

C

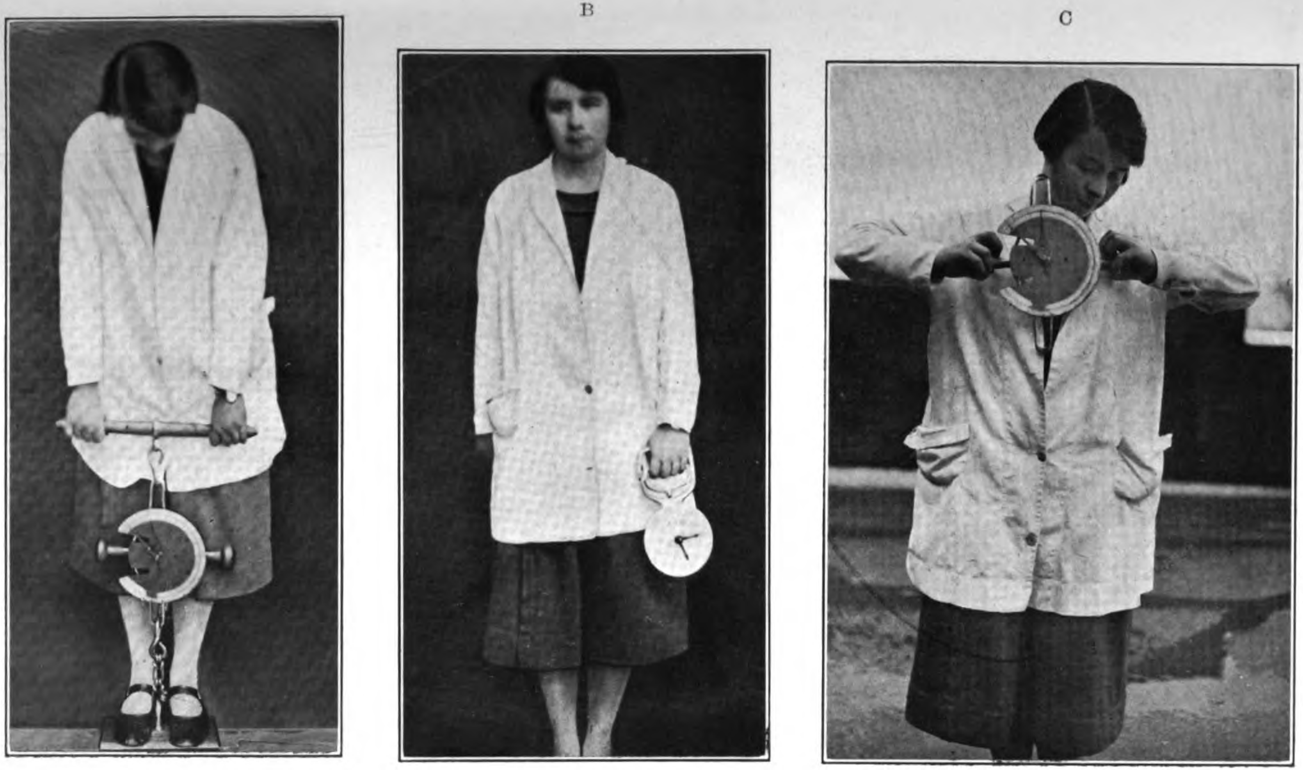

FIG. 1.

employed had done the same work before them. As evidence of what these workers could do we saw one woman who shovelled 20–25 tons of raw material, lifting it to a height of about 2·5 feet, per diem. Admittedly she was quite exceptional even in this works.

In the case of the brick works the work done was also very heavy. The girls carrying bricks, although they were permitted to select their own load, as a rule carried over 100 lbs at a time for a distance of 70–80 yards, and the women, who conveyed their bricks in barrows, had normal loads of 4 to 4·5 cwts. Exceptionally heavy work is also at times done in brick works; thus, for instance, Dr. Overton in her report records that one woman in a brick works she inspected moved 36 tons of material in the course of the working day. Here again the good carriage of the women employed was noteworthy.

The following table gives a summary of the results obtained, with their standard deviations, classified in age groups :—

TABLE II.

Age. years	No.	Mean Weight. kilos.	S.D.	Mean Height. cms.	S.D.	Mean F.T.D. cms.	S.D.	Mean Pull. kilos.	S.D.	Mean Grip. kilos.	S.D.	Mean Crush. kilos.	S.D.
14	34	38·81	6·6	151·0	7·7	56·4	3·8	63·54	14·6	20·95	4·2	17·07	5·5
15	152	44·00	6·3	155·6	6·0	58·5	3·1	72·26	15·2	23·42	4·7	19·23	6·5
16	213	46·28	6·0	156·6	5·6	59·1	3·2	78·63	15·5	25·27	4·4	20·71	5·7
17	257	47·75	6·1	157·0	5·8	59·0	3·4	79·57	13·7	25·67	4·4	21·82	6·2
18	259	50·19	6·5	158·2	5·4	59·7	3·4	83·73	14·8	26·64	4·8	23·48	6·2
19	269	50·93	7·4	158·5	5·9	59·7	3·3	85·05	16·3	26·75	4·5	24·05	6·6
20	247	50·12	6·8	157·5	5·7	59·7	3·4	83·59	15·6	26·50	4·5	23·13	6·1
21	210	49·99	6·3	158·0	5·5	59·6	3·3	85·27	16·8	27·23	5·2	23·20	6·3
22	211	49·94	6·6	157·9	6·2	59·9	3·4	85·22	18·8	26·86	5·5	22·80	6·5
23	197	49·60	7·2	157·3	5·5	59·6	3·1	84·32	17·8	27·08	5·3	22·56	5·8
24	141	50·72	7·4	157·8	6·0	59·9	3·4	88·04	16·4	27·60	4·9	24·25	6·7
25	146	49·50	7·2	156·8	6·2	59·6	3·6	85·54	18·0	26·56	4·9	23·11	6·7
26	124	50·52	8·8	157·1	6·7	59·6	3·5	86·84	17·9	27·08	5·0	23·55	7·2
27–28	164	51·26	8·3	157·8	7·0	59·8	3·8	85·17	18·9	27·60	5·0	23·62	7·4
29–30	116	50·11	6·9	156·5	6·0	59·3	3·4	82·52	17·7	26·36	4·5	22·12	6·5
30–40	232	52·94	10·4	157·3	6·7	59·5	3·3	86·98	21·4	27·19	4·8	23·47	6·6
41–55	104	54·10	9·1	156·6	5·2	59·3	3·9	80·94	21·5	25·96	4·9	19·45	5·4

It is very obvious from the above table that nearly 50 per cent. of the workers examined by us are between the ages of seventeen and twenty-two. As our sample was a fairly large one we may assume that approximately the same percentage of women engaged in industrial work will be between the same ages. It may of course be argued that, as the individuals examined were all volunteers, the younger women would be more ready to undergo the test than the older workers. Such a factor may indeed be operative, but in our opinion it cannot account for the large preponderance of young women comprised in the five years given. As a matter of fact we never had any trouble in any of the factories visited in getting a sufficient number of subjects. The nature of the test developed a spirit of competition. Fig. 2 shows very clearly the *percentage* age frequency of the employed and unemployed factory women.

As regards their body weight it will be noted that the weight rises fairly rapidly up to about the age of 18, and thereafter it remains reasonably constant until the third or fourth decades are reached. There is a definite rise in weight with the age group 30–40, and an even more marked, and not unexpected, rise in the following age group.

The height of the women examined seems to reach a level about the age of 17 or 18. It is interesting to note that after the first age group the height of the finger tips from the ground remains approximately constant (F.T.D., Table II.). This is a matter of some importance for the lay out of workshops. We took this measurement with the view of determining, if possible, the optimum height of the working bench. It would perhaps have been better, although more uncertain, to have taken the elbow height. The elbow height can be determined, however, from the finger tip distance, if there be added on the average length of forearm, as determined by Pearson and Lee. We found, using this addition, that the average elbow height of our series of workers came to 39·9 inches, a figure which agrees well with the American figure of 40 inches and that of Legros and Weston for 200 girls in this country of 39·5 inches. As the working place should be for comfort below elbow height—the arm inclined downwards at an angle of 15 to 20 degrees is comfortable, i.e. a drop of the fingers from elbow level of about 4 inches—it follows that the theoretical height of the average working bench should be about 36 inches high, and, if one to one and a half inches be allowed for the increased height of the worker due to shoe heels, the actual bench height should be about 37 inches.

As regards the strength tests it would seem from our figures that pull reaches its maximum about 18 years of age, as does also crush, whereas grip would seem to alter but little after the age of 16.

What then may be taken as the average values for women engaged in industry? The following table gives the mean values for all our subjects (3076) grouped together, and the same number separated into the official grouping of female young persons, Group I 14 to 16, Group II 16 to 18, and Group III women.

Or stated in English measure the average woman engaged in industry weighs approximately 109 lbs., is 62 inches tall, and has a pull of 183 lbs., a grip of 58 lbs. and a crush of 50 lbs.

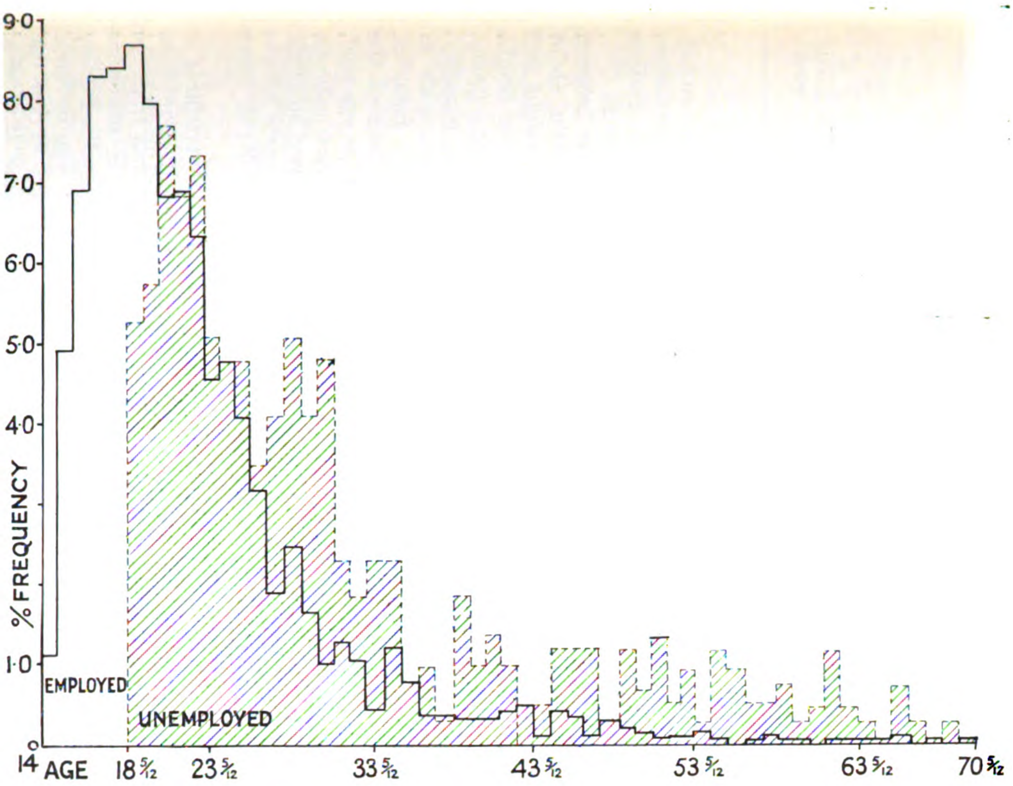

FIG. 2.—% AGE FREQUENCY OF EMPLOYED FACTORY AND UNEMPLOYED WOMEN.

TABLE III.

Group	No.	Percentage of Total	Mean				
			Weight	Height	Pull	Grip	Crush
I	399	12·97	44·76	155·8	74·9	24·2	19·8
II	516	16·78	49·00	157·6	81·7	26·2	22·6
III	2161	70·25	50·74	157·6	84·8	26·9	23·1
Total	3076	—	49·67	157·3	83·2	26·4	22·6

It was of interest to see whether any definite coefficients of correlation existed between the various physical characters measured. We found, as was to be expected, that there were high correlations between weight and height, about 0·5, and also between the various strength tests (pull and grip about 0·6, pull and crush about 0·5, and grip and crush about 0·47), but lower correlations between strength and height, about 0·35, and strength and weight, about 0·32. We found the correlation coefficients between weight and height and weight and the strength tests, as well as between height and the strength tests, to be all much higher in the first age group, of 14, than in any other. This particular group it may be remarked was small and highly selected, being for the most part girls in training in a weaving school attached to one of the factories. The fact that there is good correlation throughout between the various strength tests would seem to indicate, as might be expected, that there is, if the strength depends mainly on muscle development, a more or less uniform development of muscle in the different subjects, or, if the strength tests are indicative of more than mere muscle development (v.i.), that the factor operative is uniform for all types of muscle activity.

If we now turn to the consideration of the physical standards of the potential factory women, viz. those unemployed, some most interesting facts come to light. We obtained these women from the various Labour Exchanges in Glasgow, but found it somewhat difficult to get a sufficient number who had been engaged in factory work and who had been on the unemployed list for a year or more. Our primary object was to determine whether, when a worker had been unemployed for a year or more, there were any signs of physical deterioration. We examined 413 women between the ages of 19 and 55. The following table, in which the subjects, in order to get sufficient numbers for adequate statistical treatment, are classified in three age groups, gives the relevant data; below are given the data for appoximately the same age groups of employed women :—

TABLE IV.

Age	No.	Mean					Total Strength / Weight
		Weight	Height	Pull	Grip	Crush	
Unemployed.							
19–24	166	47·24	151·3	73·36	25·17	19·91	2·51
25–34	174	49·60	152·7	75·41	24·91	19·46	2·42
35–55	73	54·94	152·9	73·59	24·35	16·57	2·08
Employed.							
19–24	1275	50·22	157·9	84·63	26·16	23·32	2·67
25–30	550	50·38	157·1	85·09	26·94	23·15	2·68
41–55	104	54·10	156·6	80·94	25·97	19·45	2·34

And the averages for the total samples are as follows :—

TABLE V.

Group	No.	Mean									
		Weight	S.D.	Height	S.D.	Pull	S.D.	Grip	S.D.	Crush	S.D.
Employed	3076	49·67	7·7	157·3	7·4	83·18	17·6	26·44	4·9	22·60	6·5
Unemployed	413	49·60	8·8	152·2	6·8	75·07	17·8	24·91	4·9	19·13	5·7

It is then very evident, both from a comparison of the total sample and the groups classified into ages, that the unemployed women are of poorer physique than the employed. It is, however, of interest to note that taking the total sample comparison the two groups are of the same average weight, but that when the age classified groups are compared the unemployed women are found to be actually a few pounds lighter than the employed women of approximately the same age, But it will also be noted that the unemployed women, both in the total sample and in the age classified groups, are very definitely smaller in stature. It is possible then that the slightly lower weight of the unemployed is due, in the main, to their lesser height. That this is the case is shown if the weight/height ratio be taken. The values obtained are as follows :—

TABLE VI.

Weight/Height.

Age	Employed	Age	Unemployed
19–24	·32	19–24	·31
25–30	·32	25–34	·32
41–55	·35	35–55	·36

In spite of the comparatively low correlation, which was found to exist between weight and the various strength tests, it was thought that the ratio between weight and total strength might give some indication of the individual's general physique. The following table has accordingly been compiled (total strength is the sum of the three strength tests used) :—

TABLE VII.

Total Strength/Weight.

Age	Employed	Age	Unemployed
19–24	2·67	19–24	2·51
25–30	2·68	25–34	2·41
41–55	2·34	35–55	2·08

It is evident then that the ratio is smaller in the case of the unemployed. Here then we have to deal with unemployed women who, if weight is to be accepted as a criterion of nutrition, are quite as well nourished as their employed sisters, but who are both smaller and weaker. The suggestion is made that in many cases, where unemployment is prolonged, it is due to the fact that the woman's general appearance, her physique, has failed to appeal to those seeking labour. This view is supported, in our opinion, by the fact that, for instance, in one of the Exchanges with 2000 women on its lists, only about 140 of these had been out of work for over a year. The further fact that the strength of these women was below that of the employed women would almost certainly indicate that they were poorly developed rather than that they were weaker, solely because they were unemployed and their muscle flabby from lack of use. Most women have their homes to look after, and the ordinary household tasks of washing and scrubbing are much more strenuous occupations than the average industrial one, so their muscles at least are kept in reasonably good condition. Further, if there be, as is probable, a "mental" side to the exertion of strength, the poorer strength records afford additional evidence in favour of the view that the unemployed were not, speaking generally, of the average quality of the employed women.

We can now turn to the consideration of what may be called our homogeneous control group, viz. the 460 students of the Glasgow Provincial College for the Training of Teachers. This group of young women, although they lead an entirely different kind of life from the women engaged in industry, cannot be regarded as belonging to an affluent class. It is probably not overstating the matter to say that every girl in the college has of necessity to earn her own living. They are drawn from all parts of Scotland, although for the most part they belong to the West, and over 50 per cent. are country born and bred. The following table gives the results of our examination stated in four age groups :—

TABLE VIII.

Age	No.	Mean										$\frac{\text{Weight}}{\text{Height}}$	$\frac{\text{Total Strength}}{\text{Weight}}$
		Weight	S.D.	Height	S.D.	Pull	S.D.	Grip	S.D.	Crush	S.D.		
18	113	53·77	8·1	161·5	5·3	96·57	15·1	29·14	5·6	26·40	6·8	·333	2·83
19	139	52·78	7·3	160·7	5·4	96·09	16·3	27·61	5·0	26·38	7·3	·328	2·84
20	101	53·55	6·9	160·9	5·2	98·99	14·9	28·70	5·2	27·10	6·5	·333	2·89
21–22	107	52·00	7·3	160·8	5·0	100·63	15·3	28·67	5·2	26·91	6·8	·323	3·00

These young women then far exceed either of the other two groups in weight, height and strength. This is very plain when the table below giving the mean values for all three groups is examined :—

TABLE IX.

Group	No.	Mean Weight	Mean Height	Mean Pull	Mean Grip	Mean Crush
		kilos.	cms.	kilos.	kilos.	kilos.
Factory . .	3076	49·67	157·3	83·18	26·44	22·60
Unemployed .	413	49·60	152·2	75·07	24·91	19·13
College . .	460	53·01	161·0	97·90	28·47	26·66

The question of course arises whether these differences, which are found to exist, are merely the result of bad sampling, or whether they possess real significance. We accordingly worked out the probable error of the difference. If the difference found is three or more times the probable error of the difference it is regarded as being significant. When our values are considered in the light of this they show quite conclusively that, apart from the slight difference found between the mean weights of the factory women and those unemployed, all the other differences may be considered as highly significant. (Figures represent the number of times the difference exceeds the probable error.)

TABLE X.

	Weight	Height	Pull	Grip	Crush
College and Factory Women . . .	13·4 : 1	28·0 : 1	28·3 : 1	11·3 : 1	17·6 : 1
College and Unemployed Women . . .	28·4 : 1	30·4 : 1	67·1 : 1	15·5 : 1	26·0 : 1
Factory and Unemployed Women . . .	0·23 : 1	21·5 : 1	12·9 : 1	9·0 : 1	16·5 : 1

Although the college girls undoubtedly take first place in these values, determined from the means, we found that if they were compared with a very selected body of industrial workers, like those in the chemical works already referred to, they no longer take the first place so far as strength is concerned, although they are still both taller and heavier. To what are we to ascribe this superiority of physique on the part of the college girls? Presumably their stature, and to a certain extent their weight, is to be related to their stock. But the observation that their strength is so much in excess of the average women engaged in factory work is of considerable interest. It is probably due, in large part, to the fact that in the college the girls are submitted to a thorough medical examination before acceptance, are well looked after and have to undergo, as part of their normal curriculum, a course in physical training. In addition to the admirable physical training the girls are encouraged to take exercise in the form of games, like hockey and tennis. But, it is also we believe in part due to the fact that strength is not merely a function of the amount of muscle present, but it is also related to mental alertness, so that when the subject is called upon to produce some effort she co-ordinates her powers better. Mosher and Martin, in their limited studies of American women, were also impressed with the fact that in the majority of instances the mentally more alert subjects were stronger than those subjects whose activities were habitually confined to manual labour.

One may naturally ask if it is possible to deduce any standard specification, which would be capable of general application, so that a worker may be classified in terms of fitness. We have attempted many forms of calculation, but, apart from the striking uniformity which is found to exist in the total strength/weight ratio, we have obtained no very definite results. If the strength/weight ratio be taken as a criterion of physical fitness we find that the average value for the employed factory women is about 2·66, for the unemployed about 2·38, and for the college girls about 2·88. It may be hazardous to make a definite pronouncement, but, if our figures are of any value, then women with a fitness factor below 2·5 may be looked upon as probably unfitted for hard factory work, although still fitted for the lighter and less taxing occupations. Further, if the excellent physical condition of the college women is to be ascribed, as we definitely think it should, in part at least to the systematised physical training, our results should afford encouragement to those employers of labour who have included such exercises as part of their works' welfare schemes.

We have been able then in this investigation to get a good general idea of the type and the physique of the women engaged in industrial work in this country. Admittedly our sampling is limited, considering the number of women employed, and that it does not embrace the total industrial area of Britain; but, even so, our determination of the physical standards of the average industrial woman supplies data which hitherto have been lacking.

[E. P. C.]

WEEKLY EVENING MEETING,

Friday, February 4, 1927.

Sir James Crichton Browne, M.D. LL.D. F.R.S.
Vice-President, in the Chair.

Sir Arthur Keith, M.D. LL.D. F.R.S. Treas.R.I., Conservator of Museum and Hunterian Professor, Royal College of Surgeons.

Human Races, Old and New.

[Abstract.]

Sir Arthur Keith discussed certain anthropological problems which still await solution. One of these related to purity of race. Some anthropologists, he said, hold the opinion that migrations have been so rife and interminglings so free in and between all parts of the world that there now existed nowhere a race of mankind which can be regarded of pure blood. In the speaker's judgment this opinion was ill-founded; it overlooked the convincing evidence which we obtain when we inquire into the geographical distribution of mankind. Five well-marked types or stocks are recognised among the living representatives of mankind. Until recent centuries each of these occupied a definite part of the world; the yellow or Mongolian type was spread across the world from Lappland to Cape Horn, and from Greenland to the Himalayas. The type found its fullest and clearest expression in the central part of this great area—the eastern and northern parts of Asia. The white or Caucasian type was confined—until recent centuries—to that part of the Old World which lies between Ireland in the west and Beluchistan in the east; it stretched from Scandinavia to the Sahara. The Caucasian type is native to this part of the world, and we have every reason to suppose that it has been evolved in it. Between them Caucasian and Mongolian peoples, about equal as regards numbers, make up fully seven-tenths of the total population of the world. The black or Negro type had its chief distribution in Africa south of the Sahara; the most highly differentiated members of the type were found towards the west of the continent. In Asia, south of the Himalayas we find a fourth type or race—the brown or Dravidian. The fifth type, or Australoid, occupies the detached continent of Australia. However numerous or extensive human migrations may have been in long-past times, they have left the great breeding grounds of the primary races of mankind untouched.

How are we to account for the fact that each part of the world was populated by a distinctive breed of mankind? There is only one theory which offers an explanation, and that is the one put forward by Darwin. He held that the most favourable sites for the evolution of new forms were the centres of wide continental spaces. All that we know of the past and present distribution of human races favours this theory. If we accept such an explanation we must regard the Negro, the Chinaman and the European as the most recent expression of their respective types. Each we may regard as the type of being best adapted for the country and culture in which it has been evolved.

On each side of the racial frontiers of the world we find intermediate types—peoples which are partly Mongol partly Caucasian, or partly Negro partly Caucasian in their physical make-up. Such mixtures we find on the Northern and Western frontiers of India—peoples which seem to hesitate between Dravidian, Caucasian and Mongolian features. Now there has been beyond any doubt much intermarriage and intermingling across racial frontiers, but such an explanation, although accepted by the majority of anthropologists, does not account for the gradual transition which we usually find passing from the centre of one racial area to the centre of another. If we descend the Nile from its sources in Central Africa we are at first in pure Negro territory; by the time Khartoum is reached, although the Negro type has changed, it is still African; it is still negroid as far as Aswan; as we approach the Delta the type resembles more and more that of South Europe. If we continue the journey northwards in Europe the type changes until we reach the fair-haired peoples in the north of Europe. At no point is there a sharp break; we find a series of intermediate forms which carries us from Negro to Nordic. Those who seek to explain the existence of intermediate types by interbreeding of primary forms, forget that the first duty of an anthropologist is to account for the primary forms—the Nord and Negro in this case, or the Mongol and Caucasian in another. If evolution is true and Negro and Caucasian are the descendants of a common ancestor, then we ought to find, just as we do find, a series of intermediate types joining the centres where the new types have been cradled. If we trace a line on a map of the world from Norway to India we shall find it crosses a sequence of peoples which link the extreme Norseman to the Tamil of Ceylon. Between the Eskimo type of Arctic regions and the Polynesian of Tonga we find a uniting series. Once we grant the possibility of the evolution of that extreme Mongolian type, the Eskimo, and of the other extreme of the same type, the Polynesian, then there is no need to call in intermarriage to account for the intermediate types. Further, we must suppose that the extreme and most fully differentiated types represent the older and less changed forms.

Politicians and Anthropologists differ altogether in their conceptions as to what constitutes a separate race. The Politician concentrates his attention on tradition, language and spirit: the Anthropologist, following the example set by Zoologists, pays attention only to things of the body. A race, to claim that title in an anthropological sense, must have characteristics which mark it off from all other peoples. In this sense the races of Western Europe are very imperfectly separable—not because of the interminglings which have occurred, but because they are branches of the same stock, and time and space have been insufficient to permit more than a partial differentiation. In conclusion, the lecturer maintained that we can give no rational explanation of the existence of the diverse races of mankind nor of their distribution in the world unless we apply the key provided by Darwin.

GENERAL MONTHLY MEETING.

Monday, February 7, 1927.

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S. F.R.C.S.,
Treasurer and Vice-President, in the Chair.

John Lewin Callow,
Cuthbert Victor Henry Garnett,
Sir Richard A. Gregory, D.Sc. F.R.A.S.
J. E. Joseph,
Martin Hamilton Kilgour,
Sir Henry A. Miers, M.A. D.Sc. LL.D. F.R.S.
Sir Joseph E. Petavel, K.B.E. D.Sc. F.R.S.
Miss Edith Marjorie Glaholm Swann, B.A.
Miss Katie Thomas, F.R.A.M.
Christopher James Ussher,
Miss Anne Hilton Wilson,

were elected Members.

The Special Thanks of the Members were returned to Dr. J. W. Dewar, M.R.I., for his Donation of Nine Guineas to the Research Fund.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Barclays Bank, Ltd. (the Directors)—History of Barclays Bank Limited. By P. W. Matthews and A. W. Tuke. 8vo. 1926.

Dalgleish, I. S. (the Author)—The Lightning Graphs. Obl. 4to. 1926.

Hankin, Dr. J. H., M.R.I. (the Author)—Common Sense and its Cultivation. 8vo. 1926.

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M'Kendrick, Lieut.-Col. A. G. (the Author)—Applications of Mathematics to Medical Problems. 8vo. 1927.

Radio Press Limited (the Publishers)—History of Radio Telegraphy and Telephony. By G. G. Blake. 8vo. 1926.

Sir Howard Grubb, Parsons & Co.—The Scientific Papers of William Parsons, 3rd Earl of Rosse, 1800-1867. 4to. 1926.

Wright, C. T. Hagberg, Esq., LL.D. (the Author)—Nicholas Fabri de Peiresc. 8vo. 1926.

The following Publications of Scientific Societies and Periodicals have been presented or received in exchange during 1926 :—

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Synonomy of British Non-Marine Mollusca. 8vo. 1926.
Synopsis of Families and Genera of Nematoda. 8vo. 1926.
Descriptions of New Genera and Species of Lepidoptera Phalaenæ (Nocturnæ). 8vo. 1926.
Catalogue of Machaeridia. 8vo. 1926.
Guide to Exhibited Series of Insects, 4th Edition. 8vo. 1926.
Guide to Meteorites. 8vo. 1926.
Instructions for Collectors, No. 7, 5th Edition. 8vo. 1926.
Economic Series, No. 1A, 2nd Edition; No. 4A. 8vo. 1925–26.
Economic Leaflets, No 1, 3rd Edition; No. 2. 8vo. 1925.
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Report of Astronomer Royal at the Cape of Good Hope, 1925. 4to. 1926.
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Geological Survey:—
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American Geographical Society—Geographical Review. 1926. 8vo.
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Amsterdam, Royal Academy of Sciences—Proceedings, Vol. XXVII. 8vo. 1925.
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WEEKLY EVENING MEETING.

Friday, February 11, 1927.

Sir Edward Pollock, F.R.C.S., Vice-President, in the Chair.

Ernest Law, C.B.,
Author of "History of Hampton Court," etc.

Old Hampton Court Palace Revealed.

When I was honoured by the invitation of the Council of the Royal Institution to deliver a discourse on Hampton Court I felt some difficulty in deciding what particular aspect I should place before you. For this ancient, historic home of England's Kings and Queens can be considered from so many various points of view. Its architecture and construction alone, its brickwork, its stonework and stone carvings, its wood-work—carpentry, joinery and wood carving—its painted walls and ceilings would each be sufficient for a separate discourse. So would its contents, such as its five sets of superb tapestries, unequalled in Europe, fifty pieces in all, its state bedsteads of William III., Queen Anne and George II., and its rare pieces of old furniture of the same periods, its chairs, stools and tables; its cabinets, card-tables and torchères; and last, but far from least, the wonderful collection of pictures which hang on its walls. Then there are its parks and gardens and garden embellishments. Each of these would afford ample material for a separate discourse, to say nothing of the more human interest of the many events that took place within the walls of the old palace, from its foundation by Cardinal Wolsey to the end of the reign of George II.

On reflection, I came to the conclusion that the particular aspect which would probably most interest my hearers would be a survey of the portions of the Palace which have been recently discovered and revealed—especially during the last decade or two.

I must first make a brief retrospect. After the restorations of the early 'forties nothing was done at Hampton Court for some thirty-five years. But in the late 'seventies a change came over the scene by the appointment of Mr. Mitford, so well known as Bertie Mitford, and afterwards as Lord Redesdale, to the Secretaryship of the Office of Works. He was a man of taste and culture, and quickly responded to any suggestions made to him.

One of the first things he carried out was replacing the dial of the Astronomical Clock—which I had found stowed away forgotten in the Stores—to where it had been put by Henry VIII., and making it go again; which it has ever since, scrutinised with curiosity and interest by millions of visitors.

He also had the old oak doors of the Great Gate House, which I had found serving as flooring to an out-house, rehung in their original position.

Mr. Mitford, cordially supported by the then Surveyor, Mr. John Lessells, who had charge of Hampton Court and the other Royal Palaces, authorised the necessary works to carry out my suggestion to the late Sir Spencer Ponsonby-Fane, Comptroller of the Royal Household, that the old Horn Room should be opened to the public. I show you a slide of it as it is now. It is interesting as having been the receptacle for 220 years or so of Queen Elizabeth's famous collection of horns, and also on account of the old Tudor stairs, which you see on the left, discovered when the room was being prepared for opening, leading down to the Cloister, the half landing being still paved with the original tiles. It was up these stairs that the dishes were brought for the Royal Table, and taken through a great archway leading from this room to the dais in the Great Hall.

The groundwork was laid at the same time for the opening of the room called "Wolsey's Closet," which was then in the private occupation of one of the Grace and Favour residents and used as a pantry. It was arranged, with the approval of the Queen, that, when the next vacancy occurred, this interesting room should be detached from the private apartments, and access given to it from the Communication Gallery, which it adjoins. The vacancy occurred in 1886; but, unfortunately, Mr. Mitford left the office in that year, and was succeeded by Sir Henry Primrose, who, as an old Treasury official, took a very different view of things at Hampton Court, which accounts for the fact that the room was not opened to the public until 1890.

Other revelations of old Tudor work were likewise delayed. In answer to my suggestion that they might excavate the site of the old Moat, and that it would cost but little to do so, I was met with the objection "'Excavation' is not 'maintenance.'" "But," I said, "you have got that £500 a year for 'restoration of stonework and brickwork.'" "Yes, but excavation is not restoration." "But you might find some remains of Henry VIII.'s stone bridge," I said, "and then you could restore that." "Yes, if we found it; but we must find it first."

Foiled in London at the Office of Works, I had a try with my good friend, Mr. Chart, Clerk of the Works at Hampton Court, who always cordially co-operated with me in all my efforts at the Palace. I suggested to him that the drainpipe that lay along over the site might be leaking, and that for sanitary reasons he certainly ought to dig down and see whether it was all right, when he might hit on some of the stone arches I had seen some eighteen years before. But

Mr. Chart was of a somewhat timid and cautious nature, and I fancy that he, like all of them, was rather sceptical about this bridge of mine, low down under the ground—not a likely place to find anything of the sort. And so the years rolled by, and one's curiosity remained unsatisfied.

In 1894 Sir Henry was removed to a more congenial sphere—that of Chairman of the Board of Customs—and Lord Esher, then Mr. Brett, was appointed Secretary to the Office of Works. He was a round peg in a round hole, and entered keenly into everything that would add to the historic or artistic interest of Hampton Court. A great opportunity occurred in 1899, when, he having tactfully reconciled the various points of view of Queen Victoria, the Treasury and other departments, it was arranged that the State Rooms at Kensington Palace, the birthplace of the Queen, should be opened to all her subjects, in celebration of her eightieth birthday. It was necessary to find pictures and furniture to embellish the walls of some of the rooms. As a result, the King's or Cartoon Gallery, the finest perhaps of Wren's State Rooms at Hampton Court, was cleared of the tall, green screens which had long disfigured it; and the walls of Queen Anne's Drawing Room, painted by Verrio, which had been covered up for 160 years, were revealed. Some six years before I had found a rare tract in the British Museum, published in 1741, which stated that the walls had just been covered up, because the paintings were thought to be indifferent. Acting on this hint, with the permission of the Office of Works, I made an examination of the walls, which at once verified the existence of the paintings behind the papered canvas battening. One fine morning I saw the walls, pretty well as they are to be seen now, revealed to view after having been concealed for 160 years.

Lord Esher's last act at Hampton Court was the recovery of Tijou's beautiful screens of wrought iron from various museums, and the setting of them up in the King's Privy Garden, where they were originally placed by Wren's advice for William III. I discovered in the Records that it was to Jean Tijou, not Huntingdon Shaw, to whom they had hitherto been ascribed, that we owe this beautiful work, which was confirmed on my finding in the British Museum an almost unique copy of Tijou's own book, giving engravings of the screens. The slide now shown is taken from the print which was reproduced in the volume of my "History of Hampton Court Palace," published in 1891.

Lord Esher being succeeded in 1902 by the late Sir Schomberg McDonnell, nothing further was undertaken in the way of restorations or revelations at Hampton Court until 1909, when Henry VIII.'s Moat and Bridge were excavated. This was a project which I had mentioned to Lord Esher, as well as to Mr. Mitford. He was cordially in sympathy with the idea, and I have very little doubt that, had he remained at the Office of Works, it would have been carried through some six or seven years earlier than it actually was. The plan that I show is taken from the first volume of my book, published in 1885, and, as you will see, it shows the moat and bridge almost exactly as revealed by the excavations. The next slide shows the old West Front as it was, and the next the Great Gate-House and Bridge, as you have doubtless seen them yourselves, now. The next two slides give views of the Moat and Bridge looking north and south. The late Mr. Mallow's charming drawing, showing water in the Moat, always re-awakens my hope that we may yet see water flowing, if not the whole width of the Moat, at any rate as a streamlet. That the Moat was so thoroughly restored was mainly due to the great interest taken in it by Lord Harcourt, who was then First Commissioner of Works, and especially to King Edward's cordial interest and support. The work was admirably executed by the Office of Works.

Sir Schomberg McDonnell retired from the Office of Works in 1912, and was succeeded by the present Secretary, Sir Lionel Earle. With his advent a new era opened for Hampton Court, and since then, in that brief period of fifteen years, owing to his energy, taste and enthusiasm, I venture to say that, in spite of the War, more has been done to add to the historic and artistic interest of Hampton Court than in the whole previous seventy-four years since the Palace was opened by Queen Victoria free to all her subjects. I must not omit to notice here the active co-operation of Mr. Peers, the Inspector of Ancient Monuments, of Sir Frank Baines, the Director of Works, and of other Officers in the same Department. The groundwork was almost at once laid for much that I shall afterwards describe to you; though the carrying out was necessarily delayed by the War and what followed.

The first result was this official announcement on March the 25th, 1918: "The King, on the advice of the Lord Chamberlain and the representations of the First Commissioner of Works, has consented to the opening to the public of the Haunted Gallery at Hampton Court," together with a portion of Henry VIII.'s Holiday Closet, and Queen Anne's Pew, with facilities for viewing what had hitherto not been open to the inspection of the general public, the Chapel Royal, one of the most interesting of all the rooms of the Tudor Palace. The groundwork for this had been laid thirty-two years before, when, after the fire in 1886, Queen Victoria gave her sanction to the Private Apartments of which the Gallery and the Holiday Closet formed a part being, when the next vacancy occurred, done away with. This took place in the early years of this century; and I made one or two efforts to get this opening effected then; but without success.

I have described so fully elsewhere the various interesting features of the Haunted Gallery and the adjoining rooms that I will content myself here with showing you some drawings and photographs. In the process of cleaning up the Gallery, and clearing away modern excrescences, some curious old inscriptions, for centuries hidden, were uncovered and revealed. For instance, on the stone coigns and plastered jambs of an old wall of Wolsey's building, bricked up by Henry VIII. in 1535, there were discovered certain scratchings and incisions, with the names of several of Henry VIII.'s workmen, made by them just before it was bricked up. One is very plainly "G. Hone," that is Galyon Hone, the King's glazier, who was at this very time glazing the windows in this Gallery and the adjacent Watching Chamber and Hall. Another scratching is apparently the half-obliterated signature of "Richard Rydge of London, Carver," who was at the time carving the beautiful pendants in the roofs of the two chambers just mentioned. Two worthy, excellent workmen-artists they were, who did good, sound, honest work in their time. Close to these are certain roughly drawn strokes, representing a gibbet, with a man dangling from it—some joke, no doubt, among the workmen. Gibbets were commoner auxiliaries of the civil power then than they are now.

Curious is it not, to think of these old workmen thus scratching their names on this wall, little thinking, as they saw it being bricked up—presumably for ever—that their scrawlings would become objects of interest to hundreds of thousands, if not millions, of visitors in the twentieth century?

Along this Gallery Prince Edward, afterwards Edward VI., was carried to be baptized in the Chapel. Along this Gallery it was that Henry VIII. went with Catherine Howard, on All Saints' Day, 1541, to hear Mass in the adjoining Holiday Closet, where, kneeling side by side, they took the Sacrament together, the King "returning thanks to Almighty God for the good life he led, and trusted to lead, with his wife." Unfortunately, only portions remain of this Holiday Closet, most of it having been cut up and destroyed by Wren in the reign of William III. Several of its old mullioned windows, however, still exist, the stained glass, of course, long since knocked out by Puritan or classicising ruffians. A large portion of the wonderful ceiling, however, remains, mellowed indeed by age, but otherwise just as it looked on that All Saints' morning when Henry and Catherine took the Sacrament together beneath its glittering ribs and pendants.

It was along the Haunted Gallery that next morning, being All Souls' Day, Cranmer hurried anxiously—passing through the doorway now newly revealed once more—into the Holiday Closet, where Henry was again at his "kneelinge-place," Catherine Howard not being present, to slip into his hands, then folded in prayer, the paper containing the damning evidence of the Queen's gross misconduct. Along this Gallery, a few minutes after, passed the King himself, with this crushing burden on his heart, and again next day, going to the Council Chamber at the end of it, where he informed its members, with an outburst of passionate tears, of what he had learned.

It was along this Gallery that Queen Elizabeth was seen going in state to the Chapel on a Sunday in October 1584 by Leopold Von Wedel, a Prussian nobleman, who came to England in that year, and visited Hampton Court. He tells how the people standing on both sides fell on their knees wherever the glance of the Queen fell, and how she "showed herself very kindly, and accepted with a gracious mien letters of supplication from rich and poor."

I should observe that Sir Alfred Mond was at this time First Commissioner of Works, and these improvements at Hampton Court received his cordial support and aid. Indeed, I might add here that it was mainly owing to his great generosity, and the co-operation of the National Arts Collection Fund, that one of the pieces of tapestry of the Seven Deadly Sins, which had been alienated from the Royal Collection and sold for £2 in Cornwall in the 'fifties, was restored to the Royal Collection, at a cost of £6,600, generously surrendered at that—not half of its real value—by Lord Anglesey.

The next important revelation of Old Hampton Court took place on August Bank Holiday 1923, when what remains of Cardinal Wolsey's Private Rooms were opened to the public. In this case, again, it was the fulfilment of a long-deferred hope. It was as long ago as 1882 that I first examined these interesting rooms—then included in one of the Grace and Favour suites. From their position in the Tudor Palace, and the cognizances of Cardinal Wolsey in the ceiling of one of them, I identified them as those referred to in Henry VIII.'s Building Accounts as "My Lorde Cardinall's Lodgynges," a name which, curiously enough, again occurred in the Building Accounts of the time of Charles II. After that, following the Vandalistic destruction and obliteration by Wren, at the command of his Dutch master William III., of most of the State Rooms of the Tudor Palace, I have been able to find no reference to them in any of the Records. For 200 years they were lost to history, as it were; and, until I gave an account of them, with illustrations, in my "Hampton Court in Tudor Times," there had never been any mention of them at all in any historical or archæological work.

Early in the year 1914 these rooms became vacant through the death of the lady, Mrs. Carey, widow of General Carey, C.B., in whose occupation they had been since 1880. Naturally I then made an effort towards getting them revealed, as much as possible, in their original state. But at that time—with the oak panelling, which I had detected, still obscured by papered canvas-battening; with horrible nineteenth century fireplaces taking the place of the ancient ones, the rooms filled with early-Victorian furniture, did not appear to be worth while opening to the public, and were accordingly included in the grant to Lady Georgiana Peel, daughter of Lord John Russell, of First Reform Bill fame, of the suite of apartments to which they then belonged.

Fortunately, however, the recently-appointed Secretary of H.M. Office of Works, Sir Lionel Earle, entered keenly into the project of taking the opportunity afforded by the change of occupation, and examining every corner of these rooms—in which he received the cordial support of Sir Douglas Dawson, the King's State Chamberlain.

The result was very satisfactory. The oak panelling in three rooms was revealed, two being varieties of the linen-fold pattern. Two ancient stone doorways, arched and moulded, with their original huge iron hooks for hanging the massive oak doors of that period, were found and opened out, and also two fine stone Tudor fireplaces behind the modern ones.

These revelations of the old work having been carried out, and the late occupant being very generous in giving facilities for viewing the rooms, "My Lorde Cardinall's Lodgynges" became objects of much interest and curiosity to lovers of history and old English life, Her Majesty the Queen being foremost in appreciation of them. And so, when Lady Georgiana Peel died some five years ago, the King directed that they should be detached from the private suite of which they had hitherto formed a part, and be opened to the inspection of all his subjects.

All the newly revealed features in these rooms are contemporary with Cardinal Wolsey. They are the same doorways he passed through, the windows he looked out of, the fireplaces he sat by. Not often has so much antique work been brought suddenly to light in one of the King's Palaces—after an entombment of 200 years—in rooms hallowed by memories of one of the greatest figures in English history.

I show you first what we may conceive to be Wolsey's Ante-Chamber, panelled from floor to ceiling with a rare pattern of linen-fold. There is reason to suppose that it was through the door in the right-hand corner that Wolsey came forth from his private into his state rooms, as Cavendish, his gentleman-usher and biographer, describes him—"Apparelled all in red as a Cardinal, preceded by his poursuivant at arms, with a great mace of silver-gilt, and by his gentlemen-ushers, calling out: 'On, my lords and masters, on before; make way for my Lord's Grace.'"

Whether this panelling formed the original decoration of this room is uncertain. But that it was made for the room and put up at latest a very few years after the Cardinal's death is certain.

From this ante-room we pass through another doorway. It seems likely that we have here Wolsey's "Privy Chamber." The plain Tudor oak panelling was probably put up subsequently to the Cardinal's time, for his rooms were all hung with the choicest tapestries, his own more private ones being ablaze with splendid hangings of cloth of gold and silver.

One of the most interesting features of this room is the original fireplace—exactly as it was when Wolsey sat by it 400 years ago—with its back of bricks laid herring-bone-wise, still black with the wood burnt in the grate more than 200 years ago.

Entirely intact is the beautiful ceiling, one of the most attractive, and perhaps the purest in design of any in England of this period and style. It is constructed of wooden ribs, their inter-sections marked by small bosses of turned wood, with leaves of lead; while the plaques at the hexagonal radiating points are a sort of papier-mâché—called "mouldyd work" in the Cardinal's building accounts.

This room, it seems probable, is the one in which Wolsey conferred in March, 1527, with the envoys of the King of France, who came over to negotiate a Treaty of Alliance with the King of England. They had dined with the Cardinal and afterwards gone upstairs to the Queen's Chamber, where the King was, and where they were presented to him and to Katherine of Arragon. When the Queen and her ladies retired, Wolsey took the envoys down again into his private room to continue the discussion.

The upshot was the diplomatic instrument known to history as "The Treaty of Hampton Court," of which one of the chief clauses provided for the marriage of Henry's daughter, the little Princess Mary, then only ten years old, to Francis I., in furtherance of Wolsey's constant aim, indeed the very pivot of his foreign policy—perpetual peace in Europe through alliance with France.

From this room we pass into the largest of the three, but the one which has suffered most from 18th century vandalism—horrible Georgian sash windows substituted for the picturesque old bay mullioned one and others; Wolsey frieze and half of the beautiful ceiling dragged down. The half that remains exhibits the cognizances of Cardinal Wolsey—the pillars and cross-keys.

This room was formerly divided into two by a common-place partition. When removed the old fireplace was discovered. It has some curious ancient inscriptions, incised on the arched stones, not yet deciphered, but certainly decipherable.

In the corner on the left is the turret, down the spiral stairs of which Wolsey used to go in the evening into his garden. There has been laid out a Knott Garden, on what was formerly more or less a waste and untidy piece of ground. It is based on the one recently laid out at Stratford-on-Avon, these being the only Knott Gardens in England since the time of Charles I. Wolsey had a Knott Garden somewhere about here. Cavendish, his biographer and gentleman-usher, described in his metrical life of his master his "gardens sweet" with "the knottes so enknotted it cannot be expressed." Henry VIII. and Queen Elizabeth also had Knott Gardens somewhere about here. Looking from the windows we can see the very spot—the path where he walked with his chaplain saying his office, or where, to and fro, he paced with his Royal master, discussing the politics of Europe, or his Majesty's "secret business"—those "scruples" of his about Katherine of Arragon, and that want of them about Anne Boleyn.

It was in these rooms that he shut himself up in the summer of 1528, when the sweating sickness was raging around, closing his gallery and garden against everyone—Henry, the while, writing him affectionate letters, begging him to be very careful against infection, "not to eat too much supper nor to drink too much wine," and to take some pills which he had himself made up for him, and which he sent him with his love. And it was from these rooms that, on the 5th of July in the following year, he went to mount his mule in the Clock Court, to go to London to attend the final sittings of the Legatine Court, never more to set eyes on his beloved Hampton Court; and, in a few weeks, falling from his high estate "never to rise again."

I might note here that there has been at times a certain amount of confusion in regard to Hampton Court between Cardinal Wolsey and General Lord Wolseley. There is a story, absolutely true, relating to these rooms of the Cardinal's. Lady Georgiana Peel was showing a young woman, who was applying for a situation, over her apartments, and telling her that they had been formerly occupied by Cardinal Wolsey, when she exclaimed: "And to think that Lady Wolsey is still alive!"

The next portion of the old Tudor Palace to be revealed was Henry VIII.'s Great Kitchen, which was made accessible to the public on August Bank Holiday, 1925. This had been out of use as a kitchen for some 150 or 200 years, and had been allocated as a lumber-room or store-room to several successive Grace and Favour residents. The last of these was Mrs. Alexander Ellice, widow of Captain Ellice, R.N., who was granted the use of it in 1841, and retained it until her death in 1892. In connection with this Kitchen I still have a letter dated 1889 from the late Mr. Lessells, telling me that all the stores belonging to his Department, which Mrs. Ellice had harboured for them, had been cleared out of it. Mr. Lessells effected this clearance to prepare the way for the opening to the general public—which was, however, not to take place until forty years later.

On Mrs. Ellice's death, the Kitchen was transferred by the Lord Chamberlain to the care and custody of the Office of Works, who used it as a receptacle for such articles as were of archæological interest, found in the Palace— fragments of old stone carving, artistic wrought-iron work, bits of oak panelling, and decorative work of all sorts. In this way it became an object of a good deal of curiosity and interest to antiquarians, who have in recent years been afforded facilities for seeing it.

That it was opened to a larger public is only part of the policy pursued, with the cordial approval of the King, by the two Departments concerned, of affording facilities to the public for viewing the surviving remains and memorials at Hampton Court of the romantic days of the Tudors and Stuarts.

The part which is shown in the slide, and which is entered first, is only a portion—about a third—of the whole. For the accounts of the Clerk of the Works for the three years 1530–1533 make it clear that the whole building, nearly 100 ft. by 40 ft. broad, was treated and referred to as one. We are confirmed in this view when we come carefully to examine its whole plan and arrangement, for we find that the main walls of the two divisions of the Kitchen are in line, without break or straight joint, and, above all, that the roof was and is in one continuous structure.

Moreover, the fact of there being no sign anywhere of anything like an enlargement or extension of early work would seem to dispose of the idea formerly entertained—and to which, I regret to say, I too readily adopted and gave currency in my books—that we have here Wolsey's Kitchen. In any case, the whole big kitchen, as we see it now, is unquestionably that which was known in Henry VIII.'s time as his New Kitchen or Great Kitchen.

The appreciation of the opening of the Kitchen has not been confined to people in England—of whom, by the way, some 70,000 to 80,000 have visited it already. Its fame has been diffused throughout the British Empire, being described by, among other newspapers, "The Cape Times," Cape Town, "The Natal Witness," Pietermaritzburg, "The Evening Telegram," Toronto, "The West Australian," Perth, Australia, and "The Shanghai Times."

I will now show you a slide of the outside of the Kitchen; also of the adjoining "Fysshe Courte," which was cleared of some encumbrances and revealed in its present condition in the early 'eighties. I follow with a slide of the "Servynge Place," which was also cleared about the same time, the buttery hatches which you see having been hitherto entirely obscured, and a passage built alongside of the other wall.

Now, imagine the scene, on some evening when a grand banquet was being given in the Great Hall to some foreign ambassador and his suite, with all the Court assembled to do them honour to the number of several hundred persons, seated at the long tables, with our late, redoubtable, dread Sovereign Lord, King Henry, Eighth of that name, himself seated with his guests at the high table on the dais, in the middle, under the canopy of state.

Imagine the excitement, the rushing to and fro in the kitchens and offices, the eagerness that all should go well; then the minstrels, marshalled in the Serving Place, proceeding along the cloister heading the procession of dishes when, to quote the old mediæval verse:—

> "Fro Kechene came the fyrst cours,
> With pipes, and trumps, and tabours."

Having brought my narrative down to the present year, I may remark here that I am sometimes asked whether, when Henry VIII.'s Wine Cellar, the vast undercroft of the Great Hall and the "Drynkynge House," which I will describe to you in a moment, are opened, there will still remain any other portions of the Palace of sufficient historic and artistic interest to be revealed? I would answer, not any of like extent or interest, though there are some rooms here or there which might eventually be included in the circuit of these Tudor offices—for instance, the remaining third of the Great Kitchen. This so far unrevealed portion is at present in private occupation; but, at an expenditure of not many hundreds of pounds, re-arrangenents might be made which would have the double result of improving the accommodation of the Grace and Favour apartments concerned, while completing the revelation of the whole of the Great Kitchen, with more serving hatches, the original doorway into the Fish Court, and other features, including two great fireplaces, similar to those you have already seen.

Besides this there is another similar Tudor kitchen, though not such a big one, some distance away from Henry's Great Kitchen, at present mainly used for official stores, and there are some excavations which could still be made which might yield remarkable results.

These revealings of the smaller nooks and corners of the old Tudor Palace are for the future; but, with a First Commissioner so keenly interested in Hampton Court and so influential a member of the Government as Lord Peel, they will surely not be long delayed. But immediately before us, to be revealed in two or three months—largely owing, I believe, to Lord Peel's insistence—is one of the most interesting remaining portions of Henry VIII.'s Palace—namely, his "Newe Wyne Seller."

This sequel to the opening of his Great Kitchen is the more gratifying that, when once proposed, it has been carried out in record time; although, of course, one had long cherished vague hopes that it might be possible some day to have this remarkable example of Tudor domestic architecture stripped of its encumbrances, and made accessible.

It was not until four years ago that the idea was definitely broached, in connection with the problem of the vast cellarage under the Great Hall. At that time the Wine Cellar was cut up into eight or nine separate spaces, three of them in private occupation, all the rest being devoted to the storage of coke for the heating apparatus in the State Rooms. In dealing with the general problem of the whole cellarage, both in the Newe Wyne Seller and the undercroft of the Great Hall, I should like to record the very sympathetic spirit in which it was approached by Sir Douglas Dawson, the late State Chamberlain to the King, and the Officers of his Department, which was mainly concerned in it, enabling the scheme of clearance to be carried out, as I have said, in record time, alternative and more convenient accommodation being found for those affected. It was an exceedingly complex problem—indeed, something of a puzzle, but which, as it turned out, certain chance changes in occupation much facilitated the unravelling of. The final arrangements have been carried through by the present Lord Chamberlain, Lord Cromer, than whom no one has shown himself more keen to reveal all that is of historic and artistic interest in Hampton Court Palace.

I need not say that the Office of Works, on their part, cordially co-operated, by making other provision for their vast stores of coke and coal, so that they could proceed with the getting out of the encumbering partitions as soon as funds were provided by the Treasury for the purpose. Why there has been a delay in this for a year or two, and how it has come about that the expenditure was sanctioned last autumn, I will explain a little further on. The work has been going on all this winter—a very slow process, as every bit of the stuff, bricks and brick rubbish, has had to be carried out in baskets, and up steps into the open.

It is estimated that no less than 150 tons have been got out, including thousands of bricks, which have been trimmed for use elsewhere—and excellent ones they are, of considerable value. Yet such is the antiquated system of accounts prevalent at the Treasury that I understand their value cannot be set against the cost of their being got out!

The slide which I now show you is from a drawing made for me a year ago by Mr. Douglas Macpherson, mainly from measurements and data furnished to him, and by some sketching on the spot, when the cellar was still cut up and encumbered by partitions, and when one could only see a corner of the groined ceiling which is its main feature. It turns out to be almost exactly as the cellar now appears.

The treatment of the floor by the artist was necessarily very conjectural. As a result of the excavations and removal of several superimposed floorings at different levels in different parts, the original one has been revealed. But it lies a good deal lower than is shown on the drawing, so that much longer bases to the pillars are now visible. Further, the arrangements prove to be that there were gangways or paths between stands or benchings of brick, built against the walls and along the bases of the pillars, at a somewhat higher level, whereon the casks of wine rested.

Here, as always, the old accounts help us to explain fragments and remains of the original work; and these remains, on the other hand, help us to interpret what has hitherto been obscure in the accounts. For instance, there is an item in the autumn of 1535 for "takynge up the floor of the Olde Wyne Seller"—of Wolsey's time. One did not quite know what was meant by this. But the level of the floor of the Wine Cellar was evidently deliberately lowered by Henry VIII.'s architects from that of Wolsey's original cellar on the same spot; for one finds on two different walls the remains of doorways high up in the wall, and the entrance from the cloister is down several steps, the level of the cellar being four or five feet below that of the cloister. What was the exact reason for this lowering it is difficult to make out.

Another point is that there is a reference in the old accounts to making the wooden stands or stillages for the casks to rest on; and it is extremely interesting to note that the remains of these have been found in many parts of the cellar on the top of the brick benching, some of them still showing the bevelling to take the circular shape of the barrels. That they should have been preserved these 400 years is very remarkable. Of course, the Office of Works are taking the greatest care of them, on the exact spots where they were found; and so they will aid us considerably to imagine the arrangements made for stacking the casks and the general appearance when, as we may suppose King Henry went to view, as he doubtless did, his fine "Newe Wyne Seller," on his first visit to Hampton Court after his marriage to Anne Boleyn.

The pillars, and the corbels on the walls, which carry the vaulted roof are of stone, and there is an item of payment to stone-masons for "setting of pillars and chaptrells (i.e. capitals) in the Seller under the Kynges Watchynge Chamber." On the other hand, the groining itself is of brick. An item in the accounts relating to this is as follows: "Bricklayer makyng, hewing, settyng, and fenysshyng of the vought of the Kings seller under the Watchyng Chamber, conteyning in length 60 feet and 30 foot wide . . . . £37," followed by an item for "erthen pannes to kepe fyre in to dry the vought of the Newe Wyne Seller."

The steps facing one, in the picture, where men are represented by the artist as bringing in casks, lead to the cloister. Those in the right-hand corner, by the balustrade, lead down to an old Tudor stone doorway; but there has been some alteration here in the time of George II.

In regard to the next slide, the right-hand window in the round bay I believe to be a modern one. There was perhaps a door here originally. The window on the left is one of two "loop windows" in the Cellar, to which reference is made in the Building Accounts.

This Wine Cellar, together with the ale and beer cellar and the King's "Boterie," both under the Great Hall, and the Pitcher House, were subject to strict ordinances, under the general control of the "Sergeante of the Seller," who had to see that they were all kept scrupulously clean, and that the wine, beer and ale should be "good and meet drink to be spent in the Kynges house to his honour." The Officers of the Cellar had to see that the vessels were all of full gauge, and "filled full to the breames," and they were to "test the same at the brew-howse," and they had to see to the "bestowing and laying in of the beer and ale."

Other regulations dealt with the return of empty casks, and provided for "two locks to be set on every door of the cellars and buttery, the key of one of the locks to be left with the keeper of the place, and the other key to be delivered unto the Counting House."

Other regulations enjoined the brewers to "brew good and seasonable stuff, without weevil or fustines," and that they should put "neither hoppes nor brimstone in their ale, so that it may be found good, wholesome and perfect stuff, and worthy the Kynges money." Neglect of any of these regulations was severely punished —for the first time with the loss of a month's wages, and for the second time with imprisonment in the Marshalsea.

The amount of drinkables of all sorts consumed by the Royal Household was enormous, large numbers of casks, butts, etc., coming in daily, and empties going out. The cost of the whole would be equivalent in modern currency to about £50,000 a year.

There were elaborate regulations, also, for the "diett for the Kynges Majesty," for his private table in his Privy Dining Room, separate provision being made for the household. Thus there was ordered to be at dinner six gallons of beer and ale and one sextar (four quarts) of wine, and at supper the same—at a total cost represented in modern currency of £1,460 a year. The whole of the King's diet by the year is given in the ordinances as coming to about £22,000 a year in modern currency.

The Great Master of the Household, who had control of these things, allowed himself ten gallons of ale for dinner and ten gallons for supper, as well as four quarts and two pitchers of wine at each of these meals; but he only let the Lord Chamberlain have four gallous of ale and three pitchers of wine at his meals. Groom porters, yeoman and such like only got one gallon of ale at each meal.

In addition to what they could drink at the public table, a Duke or Duchess had a personal allowance of a gallon of ale in the morning, another in the afternoon, and another, with a pitcher of wine, after supper; but a Countess was allowed nothing at all after supper, and a Gentleman Usher had no allowance for the morning or afternoon.

The total of the drink and diet of the whole household came to a sum equivalent in modern currency to about £350,000 a year.

Adjoining to the "Kynges Newe Wyne Seller" is a room built, or, at any rate, altered so far as its windows are concerned, by the same King, at the same time, and called the "Drynkynge Howse." It was, in fact, the Palace canteen or "bar," to which those who had allowances of wine or ale resorted, to get their pitchers or black-jacks filled, including workmen too, who, in the good, old Tudor days, had generous allowances of free beer and "drinking times" to consume it in. I will show you a slide of it in a minute or two.

Although not a large room, being only 27 ft. long by 12 ft. wide, it is at present cut up by wooden partitions into three separate enclosures; but its original fireplace remains, though at present obscured by a common, modern grate. Some portions of the old panelling which covered its walls also remain. In my Lord Cardinal's time there was a "parlour nexte the Seller barre"—the bar-parlour, in fact—which was then hung with tapestry, and which may be identical in its main features with King Henry's "Drynkynge Howse." In due course, I have no doubt, arrangements will be made for clearing away the obstructing partitions, opening up the original fireplace, and re-covering the walls with its old panelling; and then making this very interesting relic of Tudor domestic life accessible to all who wish to view it.

Reverting to the old Wine Cellar, the interest taken in it since it was first described in the "Times" on the 22nd of June last year, has been great, universal and growing. Among others, Sir Martin Conway, the well-known art critic and connoisseur, took the matter up, and strongly urged its being opened as soon as possible. Sir William Davison also supported the idea by a question in the House of Commons, pointing out that the estimated cost of clearance, £315, could easily be recouped out of the fees charged for visiting the Great Kitchen, which brings in something like £700 a year, or by a small additional fee for visiting the old cellar itself. The answer was the somewhat mystic formula—emanating, no doubt, from the archives of my Lords of the Treasury—"Appropriation in aid cannot be used to meet expenditure"—which was evidently considered to be an unanswerable proposition, but why it was not explained.

Besides other newspapers, several of them in the Dominions and Colonies, "Punch" had a word to say upon it, remarking that the "Seller," like all best-sellers, would earn a handsome revenue if the curious public were permitted to quench its thirst, or even feast its mere eyes, on the identical spot where the eighth Henry gloried and drank deep.

Here seems to be opened up a rather good idea, which I should be glad to see carried out, and which would certainly add much to the "appropriation in aid" brought into the Public Exchequer from Hampton Court. Let the good old English name and fame of the "Drynkynge Howse" be kept up, by reviving the old bar, so that all King George's liege subjects and all visitors to his Palace may quaff good, old English ale to His Majesty's long life, happiness and prosperity, and to the immortal memory of bluff, old King Hal, in the very "Drynkynge Howse" which he, who loved his cup of wine or tankard of ale as well as any of us, built for the convenience of his household and the entertainment of strangers resorting to his Palace. Even though the authorities, in this our sordid, commercially-minded age, declined to follow the generous old Tudor custom of free allowances, even so, what better advertisement could there be for the "Come to England" movement, especially in the United States? But, whatever the authorities may decide, in order that my hearers, at any rate, may know where the "Drynkynge Howse" is, and how to get to it when it is opened, I show on the screen a view of the old Tudor Cloister, the door into it being on the left, in the corner, just on this side of the near archway.

In the meanwhile, let us, as it were, take our stand in this old Cloister, and imagine that we are in the time of James I., and that the performance of a play in the Great Hall before the Court by the "King's Company of Players"—Grooms of the Chamber to His Majesty—is just over. Torches and lanterns are gleaming and flitting about, as down the three staircases leading from the Hall the inhabitants of the Palace are streaming, some on their way to their lodgings, others to supper in the Great Watching Chamber or

Presence Chamber. And in the crowd in the cloister, among whom are gentlemen-ushers, officers of the Revels, the King's musicians, &c., none appear more conspicuous than the players, and not least, such as had not been acting that night—Shakespeare, for instance, attired in his court suit of scarlet cloth with cloak to match. Conspicuous, in a different way and from a different reason, might be Ben Jonson, in vociferous expostulation against the indignity just put upon him by having been, just before the play began, "thrust out" of the Hall by the Lord Chamberlain's "stavemen"—his "chuckers-out," in fact—for some criticisms too noisily and blatantly expressed before the assembled company. This very curious fact was discovered and communicated to me by my friend Mr. W. J. Laurence, the great historian of the English drama.

We can imagine rare Ben with the players, and the rest of them, crowding into the drinking house, and all foregathering there, and Shakespeare, the generous and tolerant, readily forgiving Ben's unmannerliness, and pledging him in a friendly cup.

It is to be noted that Shakespeare and each member of the King's Company of Actors, besides their daily allowance of a gallon of ale as Grooms of the Chamber, received an additional gallon on play nights—altogether 24 glasses of free beer!

Then wine, as well as good English ale, flowed freely, and some of the leading men of the King's Company might be privileged to quaff a cup of "Canary," or "drain a draught of Rhenish down," from the King's special wine cellar, if the "Comptroller of the Cellars" was minded to pledge a playwright or a player from one of His Majesty's choicest flagons.

Thus was it in Tudor and Stuart days. With the advent of the churlish William of Orange and the first two Georges, the old English regal hospitality and the ancient glories of Henry VIII.'s Great Kitchens, Cellars and Drinking House, passed away for ever.

But from now onwards a new and different, but a wider, fame is to attach to them, as tokens and memorials of England's historic past, to be visited in the years to come, with keen interest and appreciation I believe, by hundreds of thousands—not to say millions—of His Majesty's subjects, many of them from the Dominions overseas. Thus to project the imagination into the future, and to be able to contemplate such gratifying results, should indeed be a pleasing thought to all those who have had a share in bringing about these revelations of old Hampton Court Palace.

[E. L.]

WEEKLY EVENING MEETING,

Friday, February 18, 1927.

Sir Robert Robertson, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Sir Josiah C. Stamp, G.B.E. Sc.D. F.B.A., President of the Executive London Midland and Scottish Railway.

The Laws of Monetary Science.

[Abstract.]

The theory which dealt with money and prices had existed fairly fully developed prior to the war, and had not been appreciably modified by subsequent analysis. What had happened was the same as if a physicist had elaborated his science by reference to past experiments and records, and having no good opportunities for personal research on new and striking lines, had been suddenly presented with a fine laboratory fully equipped. The numerous changes in monetary conditions in all civilised countries, inflation, deflation, abandonment of metallic standards, re-adoption of them, changes in banking practice, had all furnished abundant illustrations and tests of theory. Such questions as velocity of circulation, which might have been regarded as abstractions before, had been graphic realities, observable almost as it were in isolation. It was perhaps on the side relating to the equation between two monetary systems that most development of monetary laws had taken place. Although Ricardo had outlined what was now called "purchasing power parity," the fuller implications of this principle had been worked out since the war, but it would, of course, become of relatively less practical importance as various countries came back to the gold standard.

The chief problems in connection with resuming a gold standard were explained, as not really whether one could by one device or another, keep enough gold in the country to maintain it, or alter the ruling rate of interest to keep the stock neither too much nor too little. These steps were relatively easy. If the values of goods measured in gold were less than the prices measured in the currency before the change, a situation would arise which was more difficult to deal with. Where all payments in gold standard currency could be immediately adjusted to have the same purchasing power all would be well. But if some important payments were not amenable to easy and early change, trouble must ensue, for the whole distribution of the flow of production to different agents would be modified, and some would get too much and others so little as not to be able to continue in their economic co-operation. We had no such means for an easy downward adjustment of wages (to secure the same real wages) in many industries. Although the gold standard was fully established here, a good deal still remained to be done, probably by hard economic disasters, in getting the consequential adjustments completed.

But the same situation could recur under a gold standard, if the gold (and the credit based thereon) altered in total at a different rate from production, and a rise in gold values (fall in prices) in the present state of wage settlements, would be a disaster. The international value of gold in its exchange for other commodities, as a standard, would need nearly as much management as any other standard, and we were not very well equipped for the task. On the theoretical side, one element in the monetary equation, the total production of goods, was still very obscure, and when we had constructed and assimilated a periodic index of production as regular and natural as our figures for foreign trade, and for births and deaths, we should elaborate much more the problem of monetary control. As a practical science such control (involving a sensitive co-operation between the central banks as "makers of money," and also their own interest in the sources of gold supply), would probably take twenty years to develop. But a closer grip of the level of gold prices keeping the fluctuations within reasonable limits was the chief solvent of trade depression, industrial disputes, and recurrent unemployment.

[J. S]

WEEKLY EVENING MEETING,

Friday, February 25, 1927.

Sir James Crichton Browne, M.D. LL.D. F.R.S.,
Vice-President, in the Chair.

D'Arcy W. Thompson, C.B. D.Litt. F.R.S., Professor of
Natural History, University of St. Andrews.

The Solids of Plato and Archimedes.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, March 4, 1927.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Secretary and Vice-President, in the Chair.

SIR HERBERT JACKSON, K.B.E. F.R.S. M.R.I.,
Director of Research, British Scientific Instrument Research Association.

Some Colouring Agents in Glasses and Glazes.

THE colouring agents of glasses and glazes are very many; moreover, several of these agents can impart colour to glasses or glazes in what may be described as different ways. An explanation which would include all our present knowledge of the behaviour of any one of them would occupy a long time. I have chosen two colouring materials, copper and iron, and hope to deal with some of the effects which can be produced by them and their compounds. If time permit I should like also to make a few remarks on certain effects which can be produced by radiations such as ultra-violet light, X-rays, and gamma-rays from radio-active materials.

I have chosen copper and iron because, in their various modes of action as colouring agents in glasses and glazes, they illustrate fairly completely the manner in which most of the materials used in this connection behave. For our purposes this evening I will not draw any definite distinction between glasses and glazes, but will use the terms somewhat indiscriminately.

Copper can be introduced into a glass or glaze so as to produce reds, browns, yellows, greens, blues, purples and black. In some instances the colours can be well seen by transmitted light, in others the material is too opaque to show the colour except by reflected light. These various effects are produced by compounds of copper or by metallic copper itself. Of the compounds of copper, attention will be especially devoted to the two oxides of copper, cuprous oxide and cupric oxide. Cuprous oxide is the colouring agent in the brilliant scarlet Egyptian glass, specimens of which appear to be correctly attributed to at least as far back as the XVIII. Dynasty and up to Roman Egyptian times. It would appear from the absence of any specimens attributable to more recent times that the knowledge of how to produce this glass successfully was then lost.

Microscopic examination of a specimen of the scarlet Egyptian glass given to me some years ago by Professor J. N. Collie, and of other specimens given to me more recently by Mr. Horace Beck, showed that the colour was due to small crystals of cuprous oxide. Chemical examination of the glass itself revealed that it was an ordinary lead glass containing about 30 per cent. of lead oxide, and the quantity of cuprous oxide varied from about 8 per cent. to 10 per cent. Based on this information the glass was successfully made after a few trials. During those trials yellow crystals were often observed in parts of the glass, and recently Mr. Horace Beck provided me with an orange yellow bead found at Faversham, in Kent, the colouring matter of which was again identified as cuprous oxide, but mainly in the yellow form. The red form of cuprous oxide is the better known form, and is produced when copper is heated in a limited supply of air ; it can generally be well seen on the underside of the black scale detached from a piece of metallic copper which has been heated for a short time to a high temperature in air and then kept at a lower temperature for some time.

A ready method of producing the red oxide is to add a solution of copper sulphate to a solution of glucose and then to add caustic potash to the mixture. On heating the clear blue solution so obtained, reduction of cupric oxide by glucose occurs, with consequent precipitation of red cuprous oxide. At the first stage of heating it is generally possible to notice the formation of a yellow precipitate mingled with the red ; these colours can be better shown by using Fehling solution, which is made up from copper sulphate, Rochelle salt and caustic potash, and is a clear blue solution. If a small quantity of the solution of glucose be poured into a large quantity of boiling Fehling solution the red oxide is formed. If a small quantity of Fehling solution be added to a large volume of boiling solution of glucose, the yellow form is produced, and is permanent in the sense that it may become rather more orange in tint but does not reach the red stage.

Without going into the doubtful composition of this yellow form, in respect of the extent to which it may be considered hydrated, it may be remarked that if it is heated out of contact with air the yellow form persists, and is apparently completely freed from water at a temperature of about 350° C. The difficulty of making any statement about the temperature at which the yellow form can exist in glass will be appreciated if a short description is given of the behaviour of cuprous oxide glass during its preparation.

If the proportion of cuprous oxide introduced into the glass be about 8 per cent., the whole of it dissolves in the glass at the temperature of 1000° C. at which the glass is made. If the glass be quickly chilled from this temperature no colour, except the almost unavoidable green colour due to oxidation of part of the copper, will be seen ; the glass is a nearly colourless transparent one. On re-heating this glass it is possible to produce, according to the temperature to which the glass is raised and the length of time during which it is heated, comparatively large crystals or aggregations of crystals of red cuprous oxide, smaller crystals of the same form, or particles so small as not to be recognised as crystals under the microscope. Along with these are frequently obtained definite crystals of the yellow form, clouds of yellow particles, and, if the re-heating be gentle, the particles of yellow cuprous oxide are so small as to be unrevealable by the microscope, and what is obtained is a clear yellow transparent glass. Here then, from two forms of one and the same oxide of copper, we have a range of colours associated with the proportions in which the two forms are mixed in the glass and with the size of their particles. What is the inner nature of the difference between the red and yellow forms which may account for their difference in colour is yet to be made out.

Much ancient Egyptian glass·does not contain lead, but is mainly of the composition of an ordinary window glass—that is to say, its principal ingredients are silica, lime, soda, with varying quantities of potash, magnesia and alumina. It has not been found possible to make the brilliant scarlet cuprous oxide glass satisfactorily in a glass of this composition. The temperature required for the proper making of such a glass is too high for the persistence of the scarlet colour, and the resulting glass is of a brown or brownish red colour. A scarlet coloured glass of the common soda-lime type could be made if the composition of the glass were so chosen that its fusing point was as low as that of the lead glass which the Egyptians used. If the Egyptians ever did make such a glass, it would have been so unstable that it could hardly be supposed any specimens of it would have persisted to the present day.

Ancient brown glass of the soda-lime type just referred to appears to have been very widely distributed. Recently I have been given some beads of this glass which, I am informed, work up through the sand on the coast of Zanzibar. Whatever attribution is given to the various specimens, they all seem to be very similar in composition and to depend for their colour upon the presence in the glass of minute particles of metallic copper. Whether the production of this brown or brownish red glass preceded that of the scarlet glass is a question which cannot be dealt with here. It is worth while, however, to refer to one property of cuprous oxide. This oxide does not form salts with acids except with the halogen acids: hydrofluoric, hydrochloric, hydrobromic and hydriodic. With the oxygen-containing acids, if it re-acts with them, metallic copper is produced along with a cupric salt of the acid. With sulphuric acid this change takes place at the ordinary temperature; with a weaker acid like acetic acid the action is very slow unless the temperature is raised. Taking these reactions as analogous to what may occur in a glass, and considering silica as a weak acid, it will be understood that if the temperature of a glass, either the lead glass or the soda-lime glass, be raised high enough the result will be the production of metallic copper dispersed through a greenish cupric oxide glass. If the percentage of cuprous oxide used were high, about that used for the scarlet glass, there would be sufficient green to make the red of copper look, by reflected light, distinctly brown. With less copper present the colour would get nearer to a red, and it will be easily understood how, by using progressively less and less copper it would be possible to arrive at a glass resembling the well-known red glass which owes its colour to particles of copper so finely comminuted as to render the glass quite transparent to the eye. The transparent red copper glass and the brilliant *sang-de-bœuf* Chinese glazes are made under conditions which tend to reduce any cupric oxide which may be formed, so that the resulting glass is not rendered dingy by a green tint, due to dissolved cupric oxide, marring the brilliant red due to the dispersed metallic copper. It should be added that for this brilliant glass so little copper is used (it is in the neighbourhood of 0·5 per cent.) that any colour from cupric oxide which would arise from such a change as is mentioned above under the action of silica would be so pale as to have practically no effect on the brilliance of the red due to the dispersed copper.

Here then again we have considerable variations in the appearance of the glass dependent upon the size of the particles of the colouring agent dispersed through it, and this is made evident when thin pieces from different portions of glaze from one and the same Chinese *sang-de-bœuf* vase are examined by transmitted light. Under the microscope, for example, some pieces are distinctly red, others are purple, others again are blue, and others look like neutral coloured glass. Closer study shows that in the most brilliant red portions no particles can be seen by the microscope. In some of the less transparent red pieces, particles are revealed either by a cloudiness or as discrete particles. In the purples these particles are larger, and in the blues they are still more marked, while in the neutral colours the particles are generally seen less closely packed but distinctly larger in size.

The colours produced by cupric oxide in glasses and glazes need not be dealt with in detail as there is much common knowledge about these. One or two points not in common knowledge may however be emphasized. In a glass of the same composition cupric oxide may give a very marked blue colour if the glass is made at a comparatively low temperature (1000°C. to 1100°C.); whereas with the same concentration of copper and the same glass made at a higher temperature, about 1300° C. for instance, there is a very marked green shade in the blue. It is worth pointing out that the blue low-temperature glass is green while hot.

It would lead us into too much detail to do more than call attention to the possible analogies between the action of water in solutions of cupric salts and the action of the oxides of the alkali metals in glasses coloured by cupric oxide. The change from blue to green just mentioned in the case of the low-temperature cupric oxide glass calls to mind the fact that a green solution of cupric chloride which becomes blue when sufficient water has been added becomes green again on heating it. A study of the changes of colour which can be produced in aqueous solutions and salts, and of the methods of modifying these colours, has been of great assistance to me in shortening the experimental work necessary to arrive at the compositions of a number of glasses in which it was desired to produce certain colours either with copper or other colouring agents.

As an example of the change of colour effected by adding water to a salt, copper sulphate may be mentioned. This salt, in its anhydrous condition or with only one molecule of water, is colourless. Crystals containing five molecules of water are the ordinary blue copper sulphate crystals, and the solution of these in water is blue. If we take boric anhydride and an oxide of an alkali metal as analogous to sulphuric acid and water, a very similar story can be told. A small quantity of cupric oxide does not dissolve in fused boric anhydride, but forms a white borate which is dispersed through the fused mass. The addition of an alkali will bring about solution and give a clear blue transparent glassy mass. The most striking example is to take boric anhydride and the alkali lithium oxide in, say, three different proportions, such as one molecule of lithium oxide to one of boric anhydride, 1 to 4 and 1 to 10. With 0·5 per cent. of cupric oxide the first is a fine deep blue; the second is a paler blue; the third is still paler; and if the proportion of alkali oxide be lowered until there is only just sufficient to bring about complete solution of the cupric oxide in the mass, there is but little colour to be seen at all.

With many glasses made at a high temperature, cupric oxide gives an olive green colour. Without going so far as to say that the dusky shade in the green is invariably due to some reduction of the cupric oxide to the lower oxide of copper, there is evidence of this in certain instances which I have come across. To take one: in making trials for a glass which was intended to be of a green colour with only a slight tinge of olive in it, and of a sufficiently light shade to enable the light of a candle flame to be seen through a 1-inch thickness of the glass, the furnace conditions happened to change on one occasion so that the glass was exposed to a reducing atmosphere. The resulting glass was so black that a bright June sun was invisible through a piece of the glass one-fortieth of an inch thick. Such a state of affairs might be considered to come about by the glass being a mixture of red copper glass mixed with a green cupric oxide glass, through which mixture but little light could be transmitted. Now red glass owing its colour to finely dispersed metallic copper is rendered colourless by fusing it and quickly chilling it. The black glass referred to might, therefore, be expected to become green if fused and quickly chilled, but it did not change from its intense black colour. This just gives a hint of the possibility of a cuproso-cupric compound being present in the glass analogous to, though not so definite as, ferroso-ferric oxide, the well-known black iron scale. Again, an analogy with solutions helps a little. If to a colourless solution of cuprous chloride in hydrochloric acid there be added a transparent green solution of cupric chloride the mixture turns black. Although dusky greens and the black glass just referred to might be accounted for by varying mixtures of red and green glass the colour of this solution could hardly be accounted for in the same way. Moreover in experimenting with red copper glasses, and studying the way in which the red colour can be prevented from developing by sudden chilling and can be produced by subsequent heating, I have repeatedly noticed that, instead of obtaining a clear colourless glass in bulbs made from the red glass and quickly chilled, the bulb has been sometimes of a dusky hue and sometimes of a definite neutral tint. As no other colouring agent but copper was present in these glasses I am inclined to attribute the neutral shade to a cuproso-cupric compound which is stable in the glass, and which is analogous to the compound formed when the cuprous and cupric chloride solutions are mixed, rather than to a physical mixture of red and green glasses.

I must not deal further with cupric oxide glasses except to mention that, unlike cuprous oxide glasses, copper glasses and many other coloured glasses, such as gold glasses, selenium glasses, cadmium sulphide glasses and opal glasses, cupric oxide glasses cannot be rendered colourless by sudden chilling, nor, indeed, can the tint of these glasses be modified to any noticeable extent in this way. An attempt to explain this difference, which seems to divide colouring agents generally into two classes, would involve a very lengthy account of the various phenomena which have been observed, and would, moreover, be to a considerable extent little more than a re-statement of facts which would involve differentiation between the meanings of such terms as "solution," "chemical combination" and "dispersion," and would lead to much argument. I must content myself with this short and incomplete account of the modes of behaviour of copper and its oxides in glasses and glazes.

Passing on now to iron, there are colours produced by ferric oxide, ferroso-ferric oxide and ferrous oxide, but I do not know of any glass or any material which could be called glassy which owes its colour to metallic iron in a fine state of dispersion. Starting in the first place with ferrous oxide, which gives the well-known green colour to glass of the nature of window glass, I need only point out that this colour is considerably modified by the composition of the glass, and without going into a number of details I will mention the way in which this tint is modified by the presence in the glass of zinc oxide or magnesium oxide. The former intensifies the colour produced by any given amount of ferrous oxide and changes the usual green into almost a blue. A similar change occurs with magnesium oxide, but the intensification of the colour is less marked. Among the alkalies, lithium oxide also tends to give a bluish tint to glasses containing ferrous iron. A notable example of this is seen in an early Chinese glaze which, through the kindness of Mr. G. Eumorfopoulos, I had the opportunity of examining. The thinner parts of this glaze are practically colourless, but the thicker parts have a beautiful blue tint. An examination of the glaze showed that it was for such glazes unusually rich both in lithium oxide and in magnesia, and the colouring material was identified as iron. If the Chinese workers had substituted zinc oxide for the magnesium oxide in their glaze mixture the colour would have been very much more marked.

Before dealing with the colours and effects produced by ferric oxide in glasses and glazes let me call attention first to the different shades of colour which can be seen in varieties of the oxide itself. These range from a reddish yellow through brick reds, bright reds to a rich brown red and almost to a black. Some specimens also have almost a bronze-like appearance. The range of colours produced when ferric oxide is used as a colouring agent for glasses and glazes is practically as great. It is doubtful if the colours produced by ferric oxide are due to compounds of this oxide with the other constituents of the glass. Without going into elaborate detail it is somewhat difficult to give adequate support to this statement. Perhaps the simplest way of dealing with it is to take the behaviour of ferric oxide in lead glasses, frequently described as flint glasses. There are light flints and dense flints. In the light flints there is always a notable quantity of an alkali such as potash or soda along with the lead oxide. In the dense flints the proportion of alkali is decreased and the proportion of lead oxide is increased. Now, taking three such glasses for example, having specific gravities of 3·2, 3·8 and 4·8, it is possible to add a known, but small, quantity of ferric oxide to the first glass and still to produce a glass having no detectable yellow colour to the eye. The same amount of iron added to the second glass will give a noticeable yellow colour, and added to the third glass will give a marked yellow colour. The glass of specific gravity 3·2 contains a notable proportion of alkali, and there is reason to believe that this either promotes the formation of a compound of ferric oxide with the alkali or the formation of a double silicate, either sodium ferric silicate or potassium ferric silicate; such compounds appear to be colourless. If the quantity of iron be increased, then a colour can be produced in the light flint, and, by increasing the percentage of ferric oxide to 5 per cent., a fairly strong yellow colour is produced. With 10 per cent. of ferric oxide in the same glass the colour is a deep brownish red when looking through a thickness of about 3 mm. With 20 per cent. of ferric oxide an even richer red colour can be seen when looking through a thickness of ½ mm.; but in thicknesses of 1 mm. or higher the glass is practically opaque. When the percentage of iron is raised much higher, some ferric oxide crystallises out from the glass on cooling, and with 40 per cent. of ferric oxide the small crystals dispersed through the glass can be seen with a hand lens. With the denser flints, containing a lower percentage of alkali, colours similar to those described above are produced with much smaller proportions of ferric oxide.

A reasonable explanation of this would be on the same lines as the suggestions made in dealing with cuprous oxide and metallic copper, viz. that the light yellow colour is due to a small amount of free ferric oxide dispersed in the glass as extremely fine particles; the transparent deep yellows and brownish reds would represent a greater concentration of ferric oxide similarly dispersed, possibly also, as the percentage of ferric oxide gets higher, as somewhat larger particles. In the 20 per cent. glass mentioned the particles are still too small to be seen, but in specimens of lead glass containing nearly 30 per cent. of ferric oxide fine clouds of almost irresolvable particles can be seen in the microscope.

Leaving the subject of lead glasses coloured with ferric oxide, I would first mention the behaviour of ferric oxide in a glass containing a large proportion of phosphoric acid, an acid which in glasses may be described as a much stronger acid than silica. Ferric phosphate is a definite compound and is colourless; in this phosphoric acid glass, quite a notable proportion of ferric iron may be present without showing any colour. Evidence that the iron is in the ferric state is obtained by heating the glass in a highly reducing atmosphere; this reduces the ferric iron to the ferrous state, and the glass is found to have developed a marked indigo blue colour, due, possibly, to ferrous phosphate, or to ferrous phosphate along with a very small proportion of unreduced ferric phosphate.

In such glasses as ordinary sodium calcium silicates it is rather difficult to obtain light pure yellow colours with ferric oxide. Much use has been made of ferric oxide in producing glazes which are of a deep colour: practically black in moderately thick layers, though of a fine golden brown in thinner layers. These glazes are frequently described as felspathic glazes, and their composition may be given as sodium or potassium calcium aluminium silicates. To get the deep colour of these glazes, amounts of ferric oxide of the order of about 12 per cent. are required. If the percentage of ferric oxide is raised to about 15–20 per cent. some of the ferric oxide separates out on cooling and, according to the concentration of the ferric oxide, so may be seen a fine brown red colour on the surface of the glazes or bronze-like spangles of ferric oxide, or rosettes, or tree-like crystals, or even more massive crystals which to the eye look black. The Chinese made much use of this behaviour of ferric oxide. As the result of a close microscopic study of certain Chinese glazes owing their colour to ferric oxide, which I undertook in collaboration with Mr. A. L. Hetherington, he was able to explain how the varied and beautiful effects seen on certain specimens of Chinese porcelain could be obtained, and to show many similar glazes produced in the laboratory to support the explanation put forward.

Ferroso-ferric oxide, magnetic oxide of iron, gives in certain glasses neutral tints of various depths, but with high concentration of this oxide a black glass can be obtained. If the concentration of the ferroso-ferric oxide be high enough, some of this oxide will come out on cooling so as to be dispersed through the glass in very minute aggregations which are, however, presumably crystalline since the resulting glass is appreciably magnetic. The above remarks refer to glasses containing the whole of the iron in the ferroso-ferric form. If a ferrous iron glass or glaze contains some ferric iron, the green colour due to the ferrous iron will be modified and various olive green tints can be obtained. There are many such coloured glazes in which the iron is principally in the ferrous state, but there is sufficient of the ferric iron present to produce the olive green tint. In a similar way small quantities of ferrous iron in a glass which contains appreciable quantities of ferric iron will modify the yellow or brown colours due to the ferric iron and give somewhat dusky hues.

Before leaving the subject of iron I am tempted to hazard a guess at the nature of the colouring matter in lapis lazuli. In almost all the specimens which I have seen, here and there crystals of iron pyrites (ferric sulphide) can be seen. An examination of these specimens under the microscope reminds me very strongly, except for the difference of colour, of the appearances of many of the glazes very deeply coloured with ferric oxide in which also crystals of ferric oxide can be seen. The colouring of lapis lazuli is very intense, so much so that very thin portions of it make equally thin portions of the deepest blue glass obtainable look almost white by comparison. Let me for a moment call attention to a simple experiment with solutions. If to an aqueous solution of ferric chloride, as free as possible from free hydrochloric acid, there be added quickly an aqueous solution of hydrogen sulphide, there is the production of a transient intense lapis lazuli colour. It has been suggested that this colour is due to the formation of a ferric sulphide which, in the solution, rapidly breaks down to a ferrous salt with the separation of free sulphur. My guess in respect of lapis lazuli is that the beautiful blue colour is due to minute particles of ferric sulphide, altogether too small to be seen by the microscope, dispersed through the mineral. Different concentrations of the dispersed ferric sulphide would account for the various lighter blues, full blues and dark, almost black, blues which can be seen in specimens of lapis lazuli. I hope to be able to find a specimen of lapis lazuli of the very blackest blue colour obtainable and to examine it critically by the microscope; I also hope to be able to test the suggestion made, by synthetic experiments.

I should mention that, in the process of making artificial ultramarine by heating together clay, sodium carbonate, sulphur and charcoal, or some other substance rich in carbon, it has been stated, I believe, by a number of experimenters that small amounts of iron must be present in the materials. I need hardly mention that almost every clay contains small, but appreciable, quantities of iron.

I have only time to deal with a few points about the third colouring agent which I mentioned at the beginning. It is quite a common thing to see glass in windows which by long exposure to sunlight has developed colour ranging from a pink, through various depths of rose-violet almost to a marked blue-violet. Some window glasses also become coloured with a brownish-yellow tint by long exposure to sunlight, but the colour is not usually noticeable. By the kindness of Dr. G. C. Simpson I am able to show a glass sphere taken from a sunshine recorder. The originally colourless or slightly green glass has become a pronounced yellow-brown; the colour of this sphere is very marked in comparison with the colourless sphere shown alongside it.

The colour developed in a glass by exposure to sunlight is partly determined by the composition of the glass. Similar discoloration can also be produced in glasses by exposing them to ultra-violet rays of shorter wave-length than those present in sunlight, to X-rays and to the radiations from radium, but the colour developed by these agencies is not, in every instance, the same as that which can be produced in the same glass by exposure to sunlight. There can be little doubt that the radiations are the cause of the colour, though what is the nature of the material which is separated out by the action of the radiations, and which produces the colour, is not fully known. In purpled window glass, manganese has been frequently mentioned as the colouring material, it being sometimes assumed that under the influence of the radiations the colourless manganous oxide in the glass becomes oxidised to a purple manganese peroxide. In a similar way it has been suggested that the brown colour such as is seen in the sunshine sphere is to be explained by the oxidation of the ferrous iron to the ferric state. There are certain difficulties in accepting these explanations of the colours. The colourless sunshine sphere shown was, a few weeks ago, a much darker yellow-brown than its companion which shows the marked brownish-yellow colour. Both spheres were colourless when new, and the present colourless one has been rendered so by heating it to a temperature of 500° C. All the varieties of coloured glass with which I have been able to experiment, and which owe their colour to the action of radiations, can similarly be decolorised by heating to an appropriate temperature; but of all the glasses I have tried which have been purposely coloured by ferric oxide, or by manganese dioxide, I know of no example which can be decolorised by heating, even if it is heated up to a temperature sufficiently high to re-melt it. This is not to say that these observations refute the suggestions of the nature of the colour mentioned above, but they certainly introduce difficulties which it would take too long to elaborate on this occasion.

Just as glasses can become discoloured by radiations so have many minerals apparently been similarly affected, and in all examples which I have tried, and in which the colours were produced presumably by radiations, the colouring could be discharged by heat. Highly coloured fluorspar is a typical example; purple amethyst, smoky quartz, rose quartz, yellow quartz, and various coloured zircons, known in the gem trade as jargons, are others. All of these can be decolorised when heated to an appropriate temperature. A fine purple amethyst, for example, became clear colourless quartz at a temperature of about 400° C. A rather browner purple amethyst lost its purple at the same temperature, but had a yellow colour left, which was discharged however at 700° C., and the resulting quartz was white but not clear; it was opalescent.

In all the instances mentioned above it is found that when the colour of the glass or mineral is discharged by heat the apparently colourless material shows a marked absorption in some part or other of the ultra-violet spectrum.

In the instances of glasses and fluorspar it is generally found that the decolorised specimens exhibit marked fluorescence under the influence of ultra-violet light. I have not seen examples of crystalline quartz which show similar fluorescence, but if the decolorised amethyst be fused, the vitreous silica so obtained shows marked fluorescence. Either the decolorised amethystine quartz or the same substance fused can be coloured again by exposure to radiations, the colour being developed most readily by exposure to the gamma-rays of radium. It is not possible, with pure crystalline quartz, which shows the highest transparency to ultra-violet radiations, to produce any coloration by submitting it to the action of the gamma-rays from radium over a period sufficient to produce marked coloration in the decolorised amethyst or in decolorised rose, yellow, or smoky quartz. Similarly I have not found it possible with fluorspar of the highest transparency to ultra-violet light to colour it by gamma-rays, but there are many specimens of fluorspar colourless to the eye which fluoresce in the ultra-violet light, and these can be coloured by exposure to the gamma-rays from radium.

From all the observations it certainly seems that the substances I have mentioned owe their colour to the action of radiations, and become coloured only when they contain a small quantity of a suitable impurity. There is no conclusive evidence of the exact nature of the material which, separated out by the action of the radiations, imparts colour to the glass or mineral. Finely dispersed metal such as calcium has been thought possible as accounting for the colouring of glass, fluorspar and Iceland spar, but glasses which contain no calcium become coloured by radiations, and it is not impossible to imagine finely dispersed potassium or sodium as the colouring material. Silicon itself has been suggested as producing the colour of some quartz, and so has titanium, and so has carbon. It is difficult to see why silicon, which is made to go back as silica by heating, should render the quartz opaque to some ultra-violet radiations. There is no suggestion which has been made which is not open to some objection or other.

In the production of these colours there appears to be some analogy with the ordinary changes which are observed in silver salts on exposure to light. A pure silver halide (chloride, bromide, iodide) is apparently unaffected by light. That the well-known action of light on these compounds as they are generally prepared is ascribable to the presence of small quantities of some other material or materials is generally accepted ; but no one would suggest with our present knowledge that the dark material which develops in the silver salt when exposed to radiation consists of the impurity. The darkening is ascribed to decomposition of the silver salt, which decomposition is rendered possible by the presence of the impurity.

Taking into account all the observations which have been made on the effect of exposing various glasses and minerals to radiations and to cathode discharge, it would appear not unlikely that the colours produced in these experiments are ascribable to the separation from the material of one or other of its constituents by the action of the radiation, which separation is only possible provided there is present in the material a suitable impurity ; but change the line of argument a little, and there are facts which would appear strongly to support the view that it is the impurity itself, in many instances, which becomes separated out in a fine state of division by the radiations and which imparts the colour to the glass or mineral. We do not know. There is a mass of evidence to be considered, but much more experimental work is needed before that evidence can be dealt with and properly assessed.

[H. J.]

GENERAL MONTHLY MEETING.

Monday, March 7, 1927.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Vice-President, in the Chair.

J. R. Beard,
Lady Biles,
William Blaikley,
Professor W. C. Clinton,
Miss J. G. Dunlop,
L. G. Glover, M.A. M.D.
H. H. Hemming,
Sir H. Cassie Holden, Bart.
James Kewley, M.A. F.I.C.
A. J. Makower,
E. L. Marler,
P. W. Rootham,
Miss Lucie Simpson,
F. S. Young, M.A.

were elected Members.

The Special Thanks of the Members were returned to Lady Dewar for her valuable gift of Two Volumes of the Collected Scientific Papers of Sir James Dewar, containingan account of the highly important researches conducted by him during the long tenure of his Professorship at the Royal Institution.

The Arrangements for the Friday Evening Discourses After Easter, 1927, were announced.

The following PRESENTS received since the last meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Coursey, Philip R., Esq., B.Sc. M.I.E.E. M.R.I. (the Author)—Electrical Condensers. 8vo. 1927.

Dewar, Lady, M.R.I.—Collected Papers of Sir James Dewar. 8vo. 1927.

Leeds Philosophical and Literary Society—History of 100 Years of Life of the Leeds Philosophical and Literary Society. By E. Kitson Clark. 8vo. 1924.

McIntosh, W. C., Esq., M.D. (the Author)—Observations on Carcinus Maenas. 8vo. 1926 (1860).

Radio Press Limited (the Publishers)—History of Radio Telegraphy and Telephony. By G. G. Blake, M.R.I. 8vo. 1926.

WEEKLY EVENING MEETING,

Friday, March 11, 1927.

J. MITCHELL BRUCE, C.V.O. M.D. LL.D., Vice-President, in the Chair.

SIR GEORGE MACDONALD, K.C.B. LL.D. D.Litt. F.B.A.,
Permanent Secretary, Scottish Education Department.

The Wall of Hadrian.

[SUMMARY.]

FOR the benefit of those of the audience who were not personally familiar with this stupendous monument of Roman frontier-defence, the lecturer began with a rapid survey of the remains, illustrated by coloured slides, and designed to convey something of the impression which the Wall and its adjuncts never fail to leave even upon the casual visitor. The course of the barrier was indicated, and each of the four elements of which it is composed was described in some detail—the stone wall itself, the ditch in front, the series of forts, mile-castles and turrets which housed its defenders, and lastly the earth-work in its rear, known to antiquaries as the Vallum. All of these presented problems to the historian and the archæologist. Within the lifetime of the present generation a great advance towards a complete solution had been accomplished. But there were fundamental questions that still awaited a final answer. The instrument through which the progress achieved in recent years had been made possible was the spade, and the three men who had wielded it most successfully were Professor Haverfield and Mr. J. P. Gibson, both now dead, and Mr. Gerald Simpson, whose invaluable work had lately received official recognition by his appointment to a specially created post on the staff of the University of Durham.

Through the labours of these three investigators, and those who had helped them, our knowledge of the Wall had been carried far beyond the point it had reached last century when the long-drawn-out controversy between the partisans of Hadrian and the partisans of Severus had been finally decided in favour of the former. It was no longer possible to accept the implications of the full-blown "Hadrianic" theory, and to regard all four elements of the barrier as contemporaneous. Successive examinations of the Vallum had provided a wealth of information as to this. It had become clear that the earth-work was certainly not older, and probably rather later, than the forts. It had become clear, too, that it was not a military work but a civil or legal boundary. The forts, again, or at least some of them, had been definitely proved to be earlier than the stone wall. Thus we seemed to get the chronological sequence—isolated forts, Vallum, stone wall. Simultaneously a careful excavation of mile-castles and turrets had been yielding important clues by which to date the various alterations and re-buildings which they and the forts had obviously undergone. The evidence of pottery and coins suggested unmistakably that there had been extensive destruction and restoration about 180 A.D., events that are certainly to be associated with the great rising in which Southern Scotland was lost. The barrier had been again swept by the besom of destruction about 270, and yet once more about 330. The final abandonment came about half a century later.

Two or three years ago, on the strength of the material that had accumulated, students of the subject were beginning to feel that it was now possible to formulate a complete and coherent theory which would explain all the phenomena satisfactorily. But they had reckoned without their host. The Spirit of the Wall had a fresh surprise in store. In 1895 Professor Haverfield and his colleagues of the Cumberland Excavation Committee had discovered at Appletree a long stretch of wall, built, not of stone, but of turf, abutting at either end on the wall proper, with which it did not seem otherwise to be organically connected. At first it looked as if the turf wall were going to provide a new and unexpected way of reconciling the rival claims of Hadrian and Severus. Presently that possibility was conclusively disproved, and there followed a disposition to leave the turf wall out of account, or at all events to relegate it to the limbo of unsolved and insoluble riddles. In 1925, however, it (or another turf wall) made a dramatic reappearance 6 or 7 miles further east at Aesica, this time on the north of the stone wall, and ultimately beneath it. That is how matters stand now. Until further digging has taken place it would be unwise to theorise further. The key will probably be found at Birdoswald.

[G. M.]

WEEKLY EVENING MEETING,

Friday, March 18, 1927.

Sir Robert Robertson, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Emil Hatschek, F.Inst.P., Lecturer on Colloids,
Sir John Cass Technical Institute.

Rigidity and other Anomalies in Colloidal Solutions.

As the term "rigidity" has colloquial and metaphorical connotations it may be advisable to begin by defining the strict technical meaning of the term. If we consider a cube of an elastic solid fixed with two opposite faces to two parallel planes, one of which is immovable, while a tangential force is applied to the other, the cube undergoes a deformation which is called "shear." It resists it by virtue of a property called its "rigidity," which causes it to return to its original shape, as soon as the force or stress ceases to act (Fig. 1).

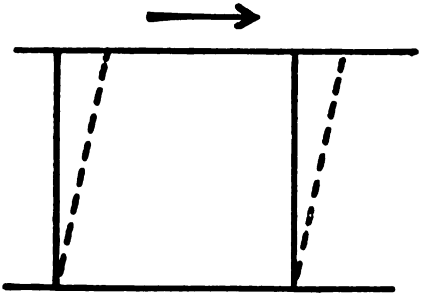


Fig. 1.

In a transparent isotropic material, such as the gelatin jelly which is being shown, a further change can be observed when the deformed body is viewed in polarized light: it becomes double refracting, and this shows itself in the familiar manner by the appearance of interference colours. This accidental double refraction, as it is called, is a very delicate means of detecting strains in transparent media, and can be used for determining their distribution and magnitude.

We can deduce a quantitative measure of rigidity from the terms of the experiment shown: the so-called modulus of rigidity, which is the force that, acting on a cube of unit dimensions, would produce unit deformation. The modulus of rigidity of steel is of the order of tons per sq. cm., that of the gelatin jelly which has been shown is about 100 grm. per sq. cm.

It must be added that the rigidity of a perfectly elastic material does not vary with the time; if a given deformation is to be maintained, the stress necessary to produce it remains the same however long it is applied.

Rigidity is one of the most characteristic properties of the solid state, and absent in all normal liquids. We can best realise the difference between solids and liquids in this respect by considering what is now merely an ideal case: two indefinitely extended parallel planes, between which a liquid is contained. If now a force, however small, is applied to one plane, while the other is kept fixed, it moves, not only a small distance as with the elastic solid, but continuously as long as the force acts, and comes to rest as soon as it ceases to do so. The force required to maintain a certain velocity is proportional to the area of the plates, the velocity gradient, i.e. the velocity of the moving plate divided by its distance from the fixed one, and depends on a property of the liquid called its viscosity. A quantitative expression again suggests itself from the conditions of this mental experiment: the force per unit area required to maintain unit velocity when the plates are unit distance apart; this is called the viscosity coefficient. In all liquids it decreases rapidly with rising temperature.

A very remarkable feature of this physical constant is that in all normal liquids it is quite independent of the velocity with which they are being sheared: if all else is kept constant, the forces required to maintain two different velocities are exactly proportional to these velocities. The proof of this constancy of the viscosity coefficient is both indirect and direct. The mathematical theory, e.g. that of the measuring devices, has been developed on the assumption of a constant viscosity coefficient, and the results agree with experiment. The viscosity of water has been measured by a number of observers over a range of velocity gradient varying in the ratio of 1 to 10,000, and has been found quite constant.

Instead of expressing the viscosity coefficient in the absolute units of the definition, it is customary in work on solutions, such as we shall consider, to express it as "relative viscosity," the viscosity of a standard liquid, generally the solvent itself, being taken as unity. Thus the relative viscosity of a 60 per cent. solution of cane sugar at 20° is 56·5.

Since, as we have seen, a normal liquid yields at once to the smallest force, no deformation like that in an elastic solid can be set up in it, nor of course the accompanying phenomenon of double refraction. Experiments with the object of detecting whether accidental double refraction could be produced in liquids by shearing them very rapidly have been made by a number of physicists, beginning with Kundt in 1881. He confined the liquid in a narrow annular space between two coaxial cylinders, one of which was rotated rapidly, and examined it in polarized light. No effect could be detected except in a few liquids which we now know to be colloidal solutions of a special type.

The arrangement of two parallel planes with liquid between them, which we have been considering, is hardly realisable experimentally, but we can, without materially affecting the reasoning, so to speak roll up the two planes, and confine the liquid between two concentric cylinders, the outer of which is rotated while the inner is at rest. This arrangement, to which we shall refer again, can then be used for determining viscosities over a great range of velocity gradients.

Historically this is not the first device used for such measurements. The systematic study of viscosity began with the work of Poiseuille, who, in 1842, found empirically the law governing the flow of liquid through a capillary tube. This "transpiration method," as it was originally called, has been very largely used since for determining the viscosity coefficients of all kinds of liquids. Thomas Graham, the founder of colloid chemistry, applied it to colloidal solutions, and was greatly struck by the changes in viscosity which they exhibited on ageing, or on addition of electrolytes, so that in his famous paper of 1863 on Silicic Acid he made the, often quoted, remark that "a liquid transpiration tube may be employed as a colloidoscope."

The transpiration tube or, as we now call it, the capillary viscometer, was subsequently used by a large number of observers, generally in the simple form given it by Wi. Ostwald, in which the pressure causing the flow is produced by a column of the liquid itself. It became evident, as measurements accumulated, that colloidal solutions did not behave like normal liquids, but the continued use of the capillary instrument produced no enlightenment, and, for theoretical reasons which need not be discussed, is inadequate to do so. On a somewhat different level is an investigation by Garrett, which forms the subject of a Heidelberg dissertation published in 1903. He studied the viscosity of various colloidal solutions both in the capillary and by a method not used before, that of the oscillating disc. If a circular, horizontal disc suspended from an elastic wire is made to oscillate round its axis in a liquid, the viscosity coefficient of the latter can be deduced from the damping effect by somewhat difficult mathematics. Garrett found in this way that colloidal solutions exhibited a number of bewildering anomalies. The viscosities found by the disc method did not agree with those obtained by the capillary; they were not consistent even by the disc method alone, but varied with the amplitude of the oscillations, and they altered during the duration of the experiment. This dissertation is quoted extensively in all the text-books published about 1910 to illustrate the complexity of the subject, but no very definite attitude towards the problem is taken by the authors.

The first deliberate attempts to bring some light into the matter were made about this time by Professor W. R. Hess of Zürich, the well-known physiologist, and by myself, working in ignorance of each other. Hess had been driven to investigate the problem from practical reasons: measurements of the viscosity of blood and serum had become a clinical method, but various observers had obtained quite irreconcilable results. Hess showed in an improved capillary viscometer that the viscosity of blood and of some colloidal solutions like gelatin was not a constant but varied with the velocity gradient; the faster the liquids were sheared, the less viscous they appeared, until above a certain velocity the viscosity coefficient became a constant.

I published early in 1911 a theoretical paper in which the behaviour of a system having the constitution which was then ascribed to colloidal solutions was investigated. The conclusion reached was that such a system would have a variable viscosity which should become constant above a certain velocity gradient. I then proceeded to test these conclusions experimentally, not in the capillary viscometer, which, from this point of view, is theoretically useless, but in a modification of the concentric cylinder apparatus first used by Couette in 1890.

It has been mentioned before that the ideal arrangement of two parallel planes can be very closely approximated by using two concentric cylinders, the space between which is filled by the liquid to be examined, and this is the principle of Couette's apparatus. Fig. 2 shows a longitudinal section through the first apparatus I used; a second and improved type is before you. The outer cylinder D can be rotated at a uniform speed, while the inner one A is suspended from a wire B and carries mirror H, which allows the deflection to be read by telescope and scale in the usual way. The effects of the bottom of D and the liquid surface must be eliminated, which is done by the two guard cylinders F. When the outer cylinder rotates, the inner one is carried round by the viscous drag of the liquid until the torsion of the wire balances this drag. Theory shows that, when the end effects are completely eliminated, the deflection divided by the angular velocity is exactly proportional to the viscosity.

The sensitivity of the apparatus can be increased to any desired extent by using thin wires, and very low velocity gradients, which are of importance in these investigations, can be employed. We have gone down to an angular velocity of 2·4° per second, i.e. one revolution of the outer cylinder in 2·5 minutes.

(The apparatus was shown working with water at an angular velocity of about 24° per second, i.e. one revolution in 15 seconds.)

The first results on colloidal solutions with the apparatus were published in 1913. Fig. 3 shows some of the viscosity curves of 0·5 per cent. gelatin solutions. The ordinates in this diagram are

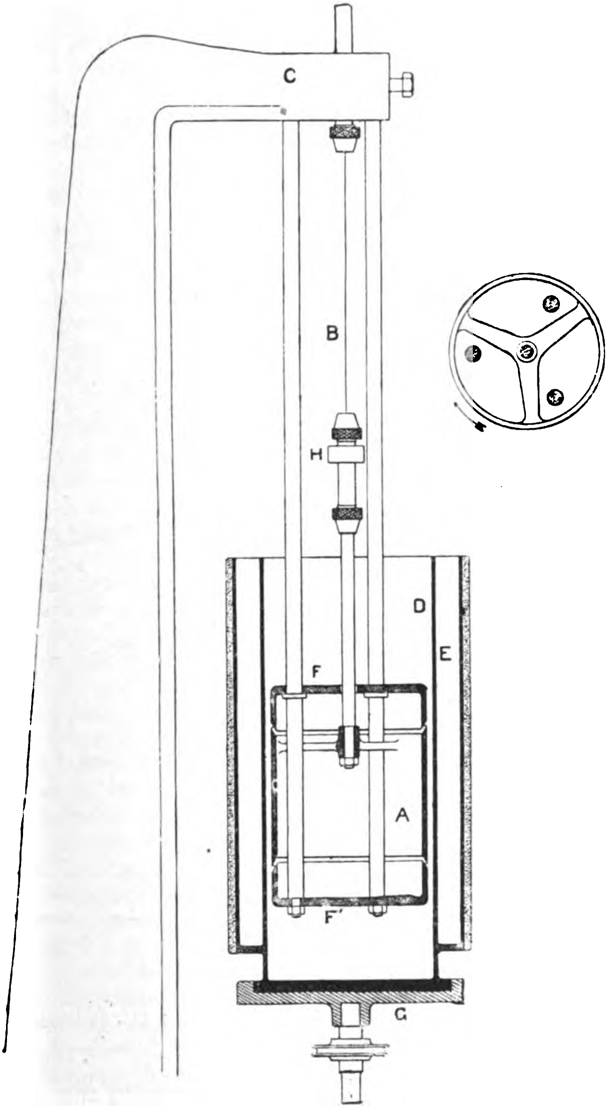


FIG. 2.

viscosities, the abscissæ angular velocities; the dotted line represents the viscosity of water, which is constant and therefore appears

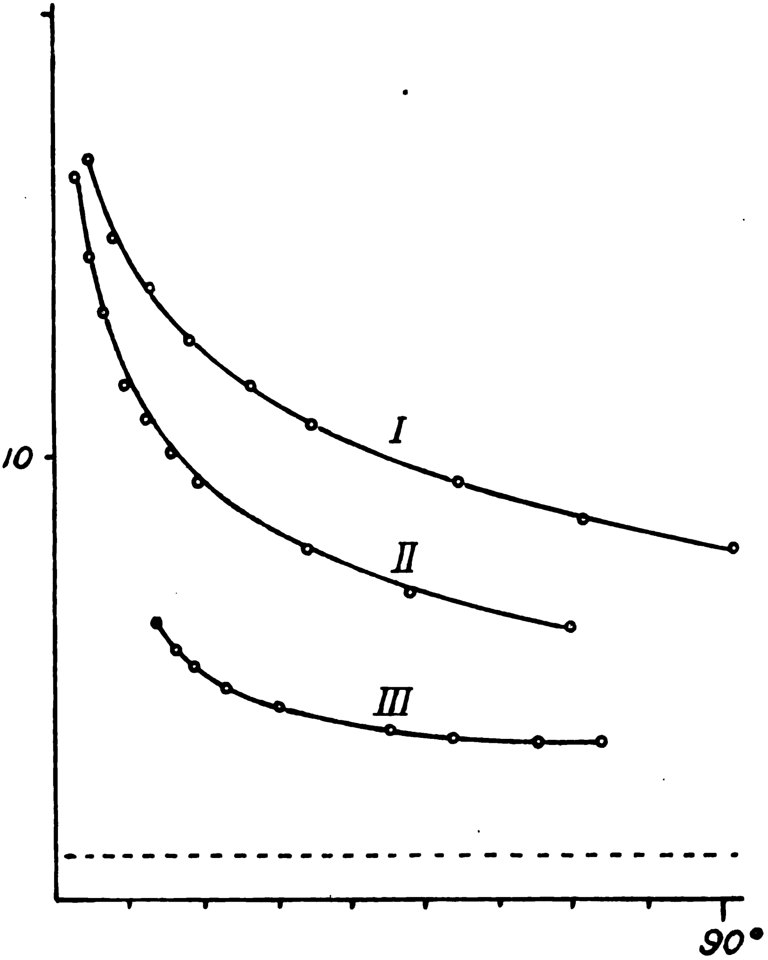


FIG. 3.
0·5% gelatin; I, 72 hours old; II, 48 hours old; III, 24 hours old.
(Hatschek, 1913.)

as a horizontal straight line. You will observe in the two upper curves that the viscosity at the lowest speed is about 2.5 times higher than at the highest speed, and that with increasing speed the viscosity shows a marked tendency to become constant, i.e. the curves become horizontal.

Many other colloidal solutions have been studied in this apparatus both by us and by Professor Freundlich, who possesses a copy of mine, and has carried out numerous investigations with his pupils since 1923. It will be sufficient to show one more set of their results, obtained by Freundlich and Schalek (Fig. 4). Sodium stearate is a soap, and benzopurpurine and cotton yellow are two dyes, with quite different constitutions and many interesting physical properties. The curves are all of the same type, and also of the same type as those for gelatin shown in the previous slide. The viscosities at the lowest velocities are again two or three times higher than at the highest, and all of them give the distinct impression of growing asymptotically with increasing velocity.

A large number of substances of the most varied chemical character have been studied by other investigators, such as agar, gum arabic and starch, rubber in various solvents, nitro cotton in various solvents, etc. It may be said quite definitely now, that they all show the same behaviour, provided the range of velocity gradient used is sufficiently wide. One observer failed to detect any anomaly in the behaviour of gum arabic, and another in that of sodium oleate ; by working at very low velocities we have however been able to show that these solutions behave like all the others, but the horizontal portion of the curve is reached at exceptionally low velocity gradients, and is therefore likely to be overlooked if this region is not examined.

Since this behaviour is thus characteristic of a very large number of colloidal solutions, and in the most striking contrast to that of normal liquids, it is of great importance to find some explanation of it. One which has been suggested by various authors, including Professor Freundlich, is that these solutions, unlike normal liquids, possess not only viscosity, but also rigidity. If this assumption is true, the liquid—to put the matter colloquially but picturesquely—will have to be stretched as well as sheared. It can be shown mathematically, by making the simplest assumption about this rigidity, that the result will be what has been found experimentally, viz. that the viscosity decreases with increasing shear.

The suggestion that solutions which nobody would hesitate to describe as liquids should yet possess one of the fundamental properties of solids is so surprising, that one is naturally anxious to have a direct demonstration rather than a mere inference from mathematical treatment. There is no difficulty in providing this demonstration with suitable apparatus—delicate and unsuitable for lecture experiments—which will be described later. I have, however, been fortunate enough to discover a solution which exhibits rigidity on such a scale that it can be demonstrated with extremely simple means in a most convincing manner. This liquid is a very dilute, about one-quarter per cent., solution of ammonium oleate, a soap familiar to this Institution as material for soap bubbles. Its rigidity can at once be shown in this simple apparatus. A cylinder, weighted at the bottom and centred in a footstep, is suspended free in the solution (Fig. 5). If a spin is given to the cylinder it will, in a

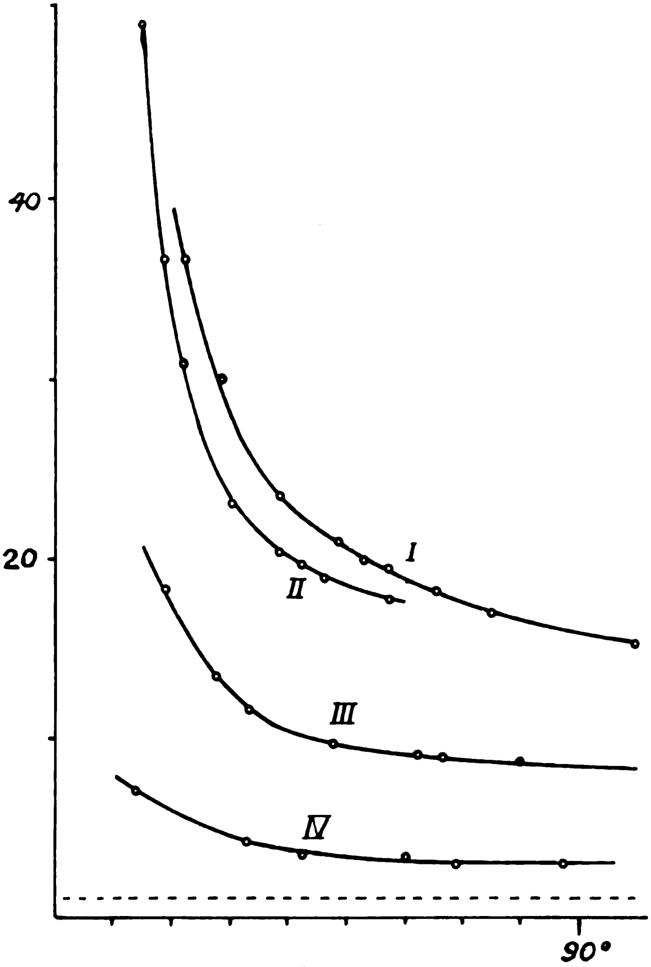


FIG. 4.

I, 0·42% cotton yellow; II, 1·0% cotton yellow; III, 0·1% sodium stearate; IV, 0·3% benzopurpurin.
(Freundlich and Schalek, 1924.)

normal liquid, revolve till the momentum is used up and then simply come to rest. The rigid ammonium oleate, however, behaves somewhat as would a hollow cylinder of rubber: the cylinder comes to rest, but is then pulled backwards.

The ammonium oleate solution is mechanically a very labile system, and its elastic properties depend very much on its treatment.

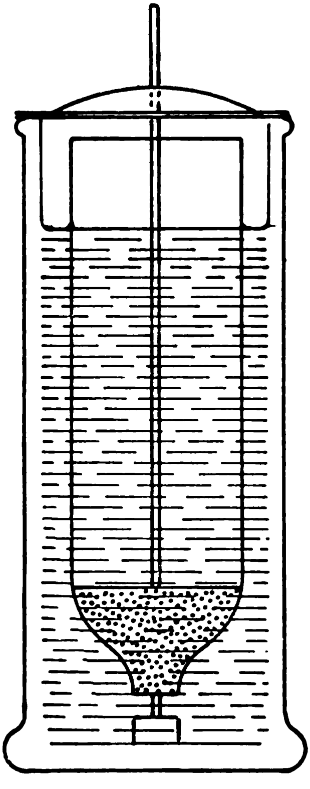


FIG. 5.

I understand they have given some trouble here in soap bubbles, and have been put down to impurities in the oleic acid. As other ammonium soaps which we have studied do not show the effect, this explanation does not seem adequate.

As the ammonium oleate exhibited such striking rigidity, it seemed to us an excellent means of testing the connection between rigidity and variable viscosity. We found that it exhibited another striking anomaly, which can be described only by the statement, extravagant as it sounds, that the solution has no definite viscosity at all. It changes constantly, and as we have shown, periodically, during a long period of measurement.

This anomaly is fortunately unique. As regards rigidity, no other solution which has yet been examined shows this effect so strikingly, and to demonstrate rigidity, and especially to make actual measurements of the modulus, much more delicate means have to be employed. Schwedoff, in pursuing the line of investigation by Kundt, developed a method and constructed an elegant apparatus, with which he carried out—or at any rate published—a single determination, that of the modulus of a 0·5 per cent. gelatin solution, in 1889. About 18 years later, Rohloff and Shinjo, at Göttingen, investigated the rigidity of gelatin solutions by a different method, and over an extensive range of concentrations, ages and temperatures. We were anxious to obtain data on other substances besides gelatin, especially on those which had been used for viscosity measurements, and used Schwedoff's method.

The artifice of employing concentric cylinders, instead of parallel planes, is again employed, and the mathematical theory is formally similar to that of the Couette viscosity apparatus. A cylinder closed at the bottom is suspended coaxially in a cylindrical vessel containing the solution to be examined by a wire of known moment; in view of the smallness of the forces to be measured very thin and long wires have to be used, and the apparatus is excessively sensitive to vibration and changes of temperature. If the wire is twisted by a known amount while the cylinder is submerged in a normal liquid, the cylinder begins to follow it at once and continues to move until it has travelled through the same angle as the wire, i.e. until the torsion in the latter has disappeared. If, however, the liquid possesses rigidity and the wire is given a certain torsion ϕ, the cylinder does not follow immediately, but only moves through a much smaller angle ω, and stays in that position for some time, the rigidity of the solution now balancing the remaining torsion in the wire. From the angles ϕ and ω and the constants of the apparatus the modulus of rigidity of the liquid can be calculated.

A great difficulty in the use of the apparatus is the necessary high degree of sensitiveness. If a liquid possessing rigidity is simply poured into it, the wire deviates considerably from the zero position, and the strains in the liquid do not disappear for many hours or even days. Fortunately all the liquids examined lose their rigidity completely at or below 40°, and recover it on cooling; the usual procedure was therefore to charge them into the beaker hot and to begin measurements on the following day.

Table I shows a number of the moduli determined with the apparatus. These have to be expressed in milligrammes per sq. cm., while, as mentioned, the modulus of the gelatin jelly shown at the beginning is of the order of grammes per sq. cm., and that of metals of the order of tons per sq. cm.

TABLE I.—MODULI OF RIGIDITY OF COLLOIDAL SOLUTIONS IN MGM. PER SQ. CM.

0·2 per cent.	gelatin, 24 hours old	0·87
0·5 ,,	cotton yellow, 68 hours old	15·8
0·75 ,,	,, ,, 20 ,,	18·9
0·75 ,,	,, ,, 68 ,,	23·8
1·5 ,,	mercury-sulphosalicylic acid, 20 hours old	2·29
1·5 ,,	,, ,, ,, 7 days old	3·24
1·5 ,,	,, ,, ,, 23 ,,	18·9
0·3 ,,	benzopurpurin, 20 hours old	0·75
1·0 ,,	,, ,, 23 ,,	11·1
0·6 ,,	ammonium oleate, 24 hours old	2·29

None of these solutions exhibit any rigidity at temperatures above 40° C.

It will be noticed that the ages of the solutions are given, and that the modulus increases with age. It has long been known that the viscosity of many colloidal solutions increases with age, and we have now demonstrated that the increase goes parallel with, and may be due to, an increase in rigidity.

Apart from this common feature, the solutions exhibit a great variety of behaviour. In some instances the presence of rigidity depends entirely on the method of preparation: thus solutions of benzopurpurine prepared cold show no rigidity whatever, only those prepared at about boiling-point. While all the solutions lose their rigidity on warming, their behaviour on cooling again varies: some are "rejuvenated," i.e. the effect of previous ageing is destroyed, while others on cooling still show their previous age.

Since these solutions can support, unlike normal liquids, a small deformation, we should expect them to show accidental double refraction when they are disturbed, and as a matter of fact most of them do so, though in very varying degrees. The effect is shown well by cotton yellow in 0·2 per cent. solution. It is even more striking in mercury-sulphosalicylic acid in 1 per cent. concentration, as was first shown by Professor Wolfgang Ostwald, to whom I am indebted for the preparation. The effect is no doubt partly due to the presence of the mercury atom.

Since the solutions lose their rigidity one would expect them to lose the double refraction at the same time, and this is indeed the case. Heating is not feasible in the cells used here, but the mercury-sulphosalicylic acid also loses its rigidity and anomalous viscosity, and, at the same time, its double refraction, immediately on the addition of sodium or potassium chloride.

Although the solutions described show rigidity to a measurable degree, the properties of the liquid state yet manifest themselves, inasmuch as they do not, like elastic solids, support deformation for an indefinite or even a long time; very soon the phenomenon called by Maxwell relaxation sets in, and in the apparatus described the suspended cylinder gradually follows the wire. In other words, the angle ω increases and the torsion in the wire decreases. The remaining torsion causes the motion of the cylinder, while the viscosity of the liquid resists it; a simple relation between the time required for equal increments of the angle and the viscosity of the liquid can be deduced mathematically, from which the viscosity coefficient at extremely low velocity gradients can be calculated. How low the velocity gradient is will be realised from the fact that in many of these determinations the time required for a travel of one degree of arc was 40 minutes. A number of these determinations have been expressed in terms of our viscosity apparatus, and correspond to one revolution of it in 180 hours, i.e. 7.5 days. All

TABLE II.—VISCOSITIES OF COLLOIDAL SOLUTIONS AT EXTREMELY LOW SHEAR GRADIENTS.

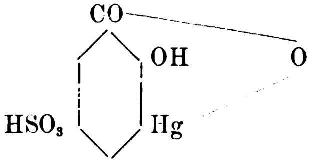
(*Equivalent to one revolution in about* 180 *hours.*)

Viscosity of Water at 20° C. = 1.

0·6	per cent.	ammonium oleate, 24 hours old . .	52,200
1·5	,,	mercury-sulphosalicylic acid, 20 hours old	55,700
1·5	,,	,, ,, ,, 7 days old	61,000
1·0	,,	benzopurpurin, 23 hours old . . .	297,300

the values at this low gradient are of the order 10,000 compared with water; to realise how high this is we may recall that the viscosity of pure glycerin at 20° is 830 times that of water. The curves obtained at very much higher velocities in the viscosity apparatus, which were shown earlier, had a tendency to become asymptotic with decreasing velocity, and the results just given fully confirm this, and indicate definitely that for infinitely small gradients the viscosity of all the solutions really becomes infinitely high.

We have thus considerable direct evidence of the existence of rigidity in the solutions which exhibit anomalous viscosity, and measurements of the latter over a very wide range, as well as the moduli of a number of them. As regards the causes of these anomalies we are still in the dark, although there has been no lack of *ad hoc* hypotheses. The characteristic of colloidal solutions is that the dissolved substance is not split up into molecules like, e.g., cane sugar in water, but into particles or aggregates containing many atoms or molecules, though still invisible in the microscope. In a number of colloids these particles can be made visible in the ultra-microscope, but the majority of those we have investigated show no particles even there. To explain the anomalies it has been suggested that the particles have peculiar shapes, and that they form threads or filaments, ramifying structures, &c. Although they are much larger than molecules they are yet supposed to be modelled on the shape of the molecule, such as the long-chain molecules of the fatty acid salts, or the long chains of amino-acids which form proteins like gelatin. As mentioned, there is little or no direct evidence of such structures, and the extreme chemical and constitutional diversity of the substances whose solutions show anomalies makes the explanation improbable or inadequate. To mention one case only : ammonium oleate no doubt has a long-chain molecule, but the mercury-sulphosalicylic acid is a simple aromatic compound of the formula



which makes chain formation difficult to conceive.

It is, however, not only the extreme diversity of molecular structures which makes such attempts at explanation unconvincing, but we have experimental evidence showing beyond any doubt that variable viscosity can be produced simply by suspending in a normal liquid a small volume percentage of microscopic particles of nearly spherical shape. I showed in 1916 with Dr. Edith Humphrey that such suspensions (rice starch in an indifferent organic liquid of the same density) exhibited viscosities which varied with the velocity gradient exactly as do the viscosities of colloidal solutions. The measurements were repeated and extended last year, and Fig. 6 shows the viscosity-velocity gradient curve of 2, 4, 6 and 8 per cent. suspensions. These curves are in all particulars similar to those found with colloidal solutions; with decreasing velocity they appear to become asymptotic to the axis, while with increasing velocity they approach, and in the lower concentrations assume, a constant value.

There is no evidence, and certainly no probability, that these microscopic particles aggregate in any way into filaments or the like, and there is thus no possibility of ascribing the variable viscosity in this case to any special configuration or arrangement. Its cause must therefore be looked for in some effect which they produce on the surrounding liquid. There is a very large amount of evidence drawn from the most diverse phenomena to show that particles in a liquid are surrounded by layers in which the properties of the liquid are altered, so that the virtual volume of the particles is increased and that of the liquid reduced. The measurements on suspensions prove that these layers must extend further into the liquid than would appear from some other evidence, and that they must be labile enough to be affected by the shearing of the liquid. These considerations apply to all particles, whatever their shape or arrangement, though the latter factors naturally add to the complications of the effect.

There is a further very strong argument for the view, that the cause of the anomalous viscosity of colloidal solutions has to be sought partly or largely in some change in the liquid, and that is the striking uniformity of their behaviour as regards temperature. Solutions of substances differing as widely as possible in their constitution behave alike, inasmuch as they lose their rigidity about the

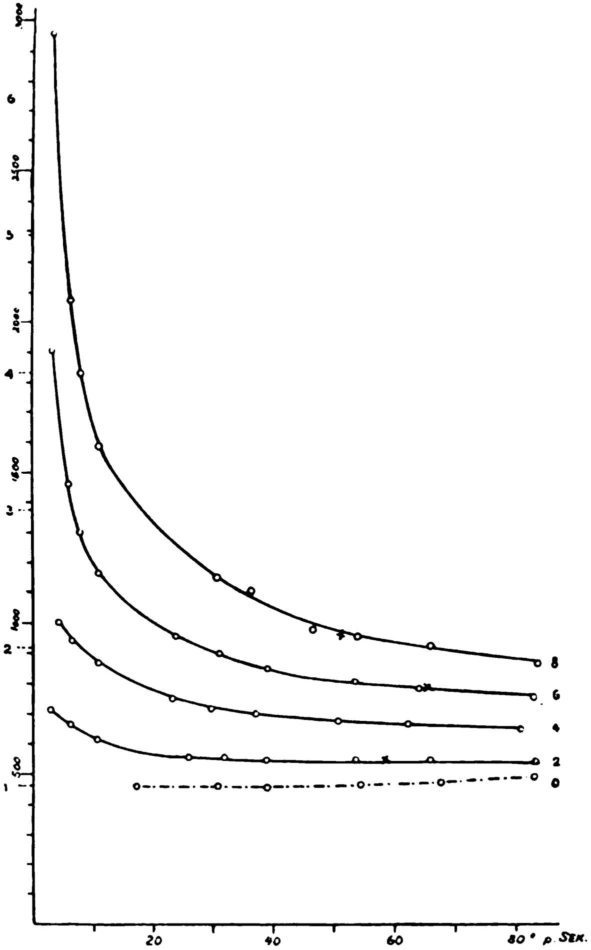


FIG. 6.

same temperature, viz. 40°. It seems natural to look for the reason of this uniformity in the factor common to all the solutions, i.e. the water, for the properties of which this temperature is significant.

It would be tempting to speculate on the significance of the anomalies which have been discussed in regard to processes in organisms, all of which consist largely of colloidal material. I do not propose to do so, but to conclude by drawing attention to the general physical aspect of variable viscosity and rigidity in colloidal solutions. It is known that under enormous pressures solids behave like liquids, i.e. flow; colloidal solutions exhibit the converse behaviour: under exiguous stresses they approximate the behaviour of solids by exhibiting enormous viscosities. Many of them pass continuously into jellies, which, as shown at the beginning, behave within limits like elastic solids, and these steady transitions recall the maxim—which it is perhaps somewhat daring to recall in these days of quanta—that "Natura non agit per saltum."

[E. H.]

WEEKLY EVENING MEETING,

Friday, March 25, 1927.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

C. T. R. WILSON, LL.D. F.R.S., Fellow of Sidney Sussex
College, Jacksonian Professor of Natural
Philosophy, Cambridge.

Thunderclouds.

MUCH may be learnt about the processes which are going on in a thundercloud by observing the sign and magnitude of the electric field which it produces at the surface of the earth and the sudden changes which lightning discharges cause in this field.

The sudden changes produced in the electric field by lightning discharges are generally of the order of 10,000 volts per metre below the central portion of the thundercloud, of the order of 1000 volts per metre at a distance of 10 kilometres, and they become comparable with the fine weather field of 100 volts per metre at about 20 kilometres. The fields destroyed by lightning discharges are most frequently negative below the thundercloud and positive at great distances. The Cambridge experiments, and those made by Appleton, Watson, Watt and Head on distant storms are all in agreement, and admit of simple interpretation on the view that thunderclouds are essentially bipolar, and that the positive charge is above the negative.

Discharge may occur between the upper and lower poles, between the ground and the lower, or more rarely the upper pole, or between the upper pole and the upper atmosphere; combinations of these also occur either simultaneous or in rapid succession.

From the magnitude of the sudden changes produced in the field by lightning discharges at known distances, the electric moments of the discharges (depending on the quantities discharged and their heights) may be determined. These generally exceed 30 coulomb-kilometres. The quantity discharged in a lightning flash is of the order of 20 coulombs. The potential difference developed in the cloud before discharge is the order of one million kilovolts, and the energy spent in a lightning discharge is about 10^{17} ergs or 10^{10} joules.

Recent work on the propagation of electromagnetic waves has proved very definitely (what the facts of terrestrial magnetism had long seemed to indicate), that the upper atmosphere is highly ionised.

Thunderclouds are electric generators of very high voltage situated between the earth and the conducting layers of the upper atmosphere, out of which they can draw a plentiful supply of ions. There are probably both continuous currents and momentary discharges between the cloud and the upper atmosphere; between the lower pole and the ground current is carried by charged rain, by lightning and by point discharges. If thunderclouds are mainly of positive polarity (i.e. if the upper charge is positive), they must maintain an upward current, but between the earth and upper atmosphere. Nearly 2000 thunderstorms are on an average in action at a given time; it is quite possible that they may be the main sources of the downward current and positive potential gradient of fine weather regions.

In a thundercloud, on account of the great distance through which the intense electric fields extend, effects may be possible which we cannot hope to produce in the laboratory. It may for example be possible for electrons to be accelerated till their energy is some hundreds of times as great as that of any known beta particle.

[C. T. R. W.]

WEEKLY EVENING MEETING,

Friday, April 1, 1927.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Vice-President, in the Chair.

John Allen Howe, O.B.E. B.Sc. F.G.S. M.Inst.M.M.,
Assistant Director, Geological Survey.

The Stones of London.

The stones of London, like those of any great city, may be considered from different points of view, historical, sentimental, technical. Members of this Institution have shown themselves as much interested in art and the arts as in science, and it might be appropriate to discuss the stones used by prehistoric man that are found on this site; or the history of certain unique stones such as "London Stone," sheltered behind its grille on the wall of St. Swithin's Church; the "Stone of Destiny" at Westminster; the King's Stone at Kingston; or the stone from Old London Bridge, forming the seat of the chair of the Master of Fishmongers' Hall.

I shall be on safer ground if I restrict my remarks to the building stones of London; not so much considering those of the past as those of the present and future.

Hardly anything new can be said about building stones, though much has been written on the subject. People have been carving limestone for 5,000 years—witness the beautiful fragment recently found at Ur—and the ancients wrought many hard stones; yet to-day the masons of London hesitate to tackle stones that were carved by the early Egyptians.

I propose to discuss briefly the Selection, Decay and Preservation of building stones as applied to London, and to place before you on the screen a few facts relating to these subjects.

Selection.

In certain favoured places the abundance of suitable local stone has settled the question of choice in a simple manner. The prevalent colour, and to some extent the style, of such towns as Aberdeen, Bath and Chester has been determined in this way.

London, situated on a substratum of clay and incoherent gravel, from its very beginnings has been compelled to go beyond its borders for stone. So great has been the destruction and reconstruction in

London that little of the oldest masonry remains; yet in the fragments of the Roman Wall we have evidence that its builders sought the slopes of Hertfordshire for boulders of sarsen stone, the Downs and Weald of Kent and Surrey for chalk, ragstone, chert and malmstone; they used the septarian nodules from the London clay, and the local flint gravel was freely employed then as now.

The Normans introduced the stone from *Caen*, of which examples may be seen in the Tower, Westminster Abbey and other buildings. *Beer* Stone from Devon and the somewhat similar *Clunch* Stone from the Chalk formation were much in demand for carving from about the 11th century; while for columns, beautifully exemplified in the Temple Church, the marble of Purbeck was the favourite material.

From the Great Fire of 1666 a new era began for London; although many other stones have been and still are employed, Portland Stone from that time began to play a dominant part. Apparently introduced by Inigo Jones, its use was thoroughly established by the extraordinary activity of Sir Christopher Wren.

Time will not permit me to deal with the merits of the many kinds of stone that have been employed in London, nor to mention them all by name. As types I may refer you to Bath Stone at Apsley House and the new Canada House; to Ancaster Stone at the Surveyors' Institute; to Ketton Stone in Dean's Yard, Westminster; to Doulting Stone at Holy Trinity Church, Sloane Street. Granite from Cornwall is seen in the old home of the British Medical Association, Strand; from Scotland in the Duke of York's Column; from Norway at the Morning Post building and Ritz Hotel. Trachyte from Bowral forms the plinth course at Australia House; marble from the Apuan Alps forms the Marble Arch; Pentelic marble from Greece is found at the top of St. James's Street, and from Norway at 101 Piccadilly. To-day Portland Stone is finding a competitor in "French Portland," mostly from Coutarnoux quarries in Burgundy; while both Portland and other limestones are being challenged by artificial products: terra-cotta has already made its mark, and "cast stone" is finding considerable employment.

The need for knowledge of the wearing qualities of stone has given rise to an immense amount of research in the past, in the desire to discover reliable means of estimating the relative durability of untried stones. The obvious method of comparing the behaviour of stones in existing structures cannot be applied to stone from a new source.

In selecting the stone for the Houses of Parliament the Royal Commission of 1839 adopted this excellent method, and examined a large number of buildings throughout the country; they recommended a stone which had been proved to stand well. But it is not enough to select the stone unless the quarries can produce the right quality at the required rate.

The results of chemical, physical and mechanical tests, with freezing, soaking and microscopical examination, have been accumulated by the labours of Bauschinger, Baldwin-Wiseman, Hudson Beare, Bissinger, Boheme, Gary, Hanisch, Hirschwald, Kessler, Pratt, Parker, Schenck, Seipp, Tetmajor and others.

Hirschwald, starting from the firm ground of actual results of long exposure of stones in ancient buildings, examined and tested the stones to determine their properties, which he analysed with great elaboration, and from the results attempted to derive formulæ by which new stones could be placed in order of relative durability. His methods were sound, but the results are disappointing ; there are too many factors difficult of exact evaluation. While each type of stone, limestone, sandstone, granite, etc., had to be measured by its own criteria, there were so many subordinate and rather vague characters to be integrated, peculiar to each type, that the scheme is hardly workable.

The charm of stone rests largely on the fact that every block has some individual characters, but this very fact makes selection and testing the more difficult.

It is possible to forecast to some extent the weather-resisting properties of a stone from microscopic examination and from some of the recognised methods of testing : it is possible to do so only imperfectly.

Many years are required to test fairly the comparative wear of different types of stone. In order to afford some evidence on this subject the Royal Institute of British Architects, through its standing Science Committee, arranged with the Geological Survey and Museum in 1910 for the continual exposure of a few selected types of stone under similar conditions of environment. Some of the effects of about 10 years' weathering are shown in the series of photographs in the library. In practice there are other difficulties ; though an architect may desire stone of a specified colour, texture or hardness, his choice is usually restricted by what is on the market, and it may be further influenced by what his client is prepared to pay, and occasionally by trade antagonism or local jealousies.

DECAY OF STONE.

The decay and disfigurement of limestones in town buildings usually takes one of the following forms :—

(1) Normal weathering, a process of etching, resulting in the gradual recession of the exposed surface.

(2) Blistering, commonly due to the growth of sulphate crystals behind a surface skin.

It is important to observe that this disease of stone is essentially a product of civilisation; one may walk by hundreds of miles of rough limestone walls or limestone escarpments and see no trace of it. The trouble is characteristic of smoothly dressed stone on which a toughened, hardened or waterproofed surface has been produced either by "seasoning," by which is meant the slow movement of the quarry sap to the surface; by the compressive action of the tool in dressing; by the application of preservative solutions and waterproofers; or by the accumulation of a layer of oily soot.

So long as the hardened skin is intact and there is no entry of injurious solutions to its rear no harm will be done; but once the acid moisture gains access at any point, that point becomes a centre of corrosion which grows behind the sheltering surface until the pressure of its growth bursts through the protective film.

(3) Corrosion without blistering, by efflorescence of sulphate or other crystallising salts.

This is a very common trouble due to the leaching of crystallising solutions from the mortar or from the ground. It may also be caused by the water from earth behind retaining walls or by rubbish left in contact with the stone for a period during construction. In damp weather the salts travel about on the surface of the stone, working their way into the subsurface layer, and when a drier time comes they crystallise out, forming obvious and unsightly white patches, which roughen the surface and may eventually form deep pits. There is at the moment a bad case on a bank in Pall Mall.

Such patches may continue active for years, or they may last for only a short period if the salts are removed by rain or washing, or their source becomes sealed off. Interiors are not immune, where there are gas or coke fires to yield the sulphurous fumes.

(4) Rotting and crumbling caused by acid moisture encroaching on the interior of the stone.

This rarely takes place on a well-exposed sound type of stone unless the first stage has been as in (2).

(5) Fracture or gashes caused by the gradual development of planes of weakness, usually joint or bedding planes.

Few good stones suffer badly in this way. Those with strongly marked bedding structure, like Ham Hill Stone, may fracture along the bedding, particularly if face-bedded. The most noteworthy example of joint-fracturing in London is that of the magnesian limestone in the Houses of Parliament, where, owing to the great extent of the edifice and the character of its ornament, the trouble is very serious.

(6) Fracture due to the expansion caused by the rusting of iron dowels, tie-rods and hoops used in construction. Here again there are many examples in the Houses of Parliament.

Decay in sandstones occurs mainly in the form of scaling, splitting, rotting and wind-etching.

Scaling, or the separation of flakes from exposed surfaces, is independent of the natural bedding of the stone and follows the surface contours of the block in mouldings, steps and balusters. It appears to be primarily due to alternating conditions of moisture, assisted by frost and sometimes by the access of sulphate solutions.

Splitting takes place parallel to the natural bedding, and is most evident in stones with well-developed bedding, or those in which there is a latent tendency to split caused by the regular orientation of mica flakes.

Rotting is a failing of calcareous sandstones; it may also be induced by solutions of salts. As long ago as 1839 it was observed that in several cases sandstone plinths or string courses, when liable to drip from limestones, were in an abnormal state of decay. Quite recently Mr. Schaffer at the Building Research Station has demonstrated that in certain cases this decay has been accelerated by the absorption of sulphate solutions from the weathering of limestone overlying the sandstone.

The decay of granite is normally very slow in London. An interesting example of the effect of damp combined with the skin-forming action of hammer-dressing is exhibited by the plinth at Goldsmiths' Hall. The whole face of some of the 3 ft. blocks has come away.

One source of decay, greatly expanded during recent years, is the prevalence of pigeons with all that their presence implies. Note the crinolines of wire now required to protect Corinthian capitals.

PRESERVATION OF STONE.

The obvious and occasionally rapid deterioration of stonework led long ago to efforts which aimed, on the one hand, at conserving what remained of old decayed structures, and, on the other, at fortifying new stonework. In neither case has real success been achieved.

The need for conservation is more pressing in towns than in rural surroundings, because in towns a greater degree of finish is expected to be maintained.

So insistent has been, and still is, the demand for action of some kind that a host of preparations and so-called preservatives have been proposed and put to use. The subject has recently been dealt with in a masterly manner by Mr. Noel Heaton in a lecture before the Royal Society of Arts, and by Messrs. Fox and Harrison of the Government Laboratory.

These preparations embrace, amongst other things, resins, gums and waxes, casein and oils compounded in a variety of ways; alkaline and other silicates, baryta and colloidol barium sulphate, various silico-fluorides, and the newest, silicon ester, resuscitated by Dr. A. P. Laurie, who has long striven to solve the problem of preservation. Besides preservation there must be repair, and this may be effected by patching with natural or artificial stone or by mastics.

The cause of ultimate failure in the case of most of the preservative processes is the same; the penetration of the agent beneath the surface of the stone is so slight that, even when it is not a mere daub of covering material, the result is still in effect no more than a skin treatment. If the skin were permanent, and if it had no effect on the appearance of the stone, its object would be attained. The skin may last a long time in favourable conditions, but when once the weather gains access, as may soon happen in the vulnerable parts of the building, it becomes a source of danger, hiding the destruction which is proceeding behind it all the more rapidly for the shelter it gives, and finally resulting in a sore more serious than could have been formed without its aid. Thus, to be effective, the application must be renewed at sufficiently frequent intervals.

The sort of preservation we want is one which while it enables the stone to retain its form will also maintain its surface texture and characteristic colour. The silicon-ester treatment or the silicofluorides are fairly satisfactory in so far as they do not seriously impair the appearance of the surface; but the former is too new to be judged on results, and the latter requires repeated applications; both are liable to be ineffective preservatives or skin-formers owing to lack of penetration.

It is instructive to note what is being done by way of stone "renovation" in Piccadilly. Starting at Hamilton Place we find a Portland Stone building treated about 1914 by a proprietary process long in use in this country and in France. The appearance is by no means unsatisfactory, but the texture is no longer that of Portland Stone; the condition generally is still good, but decay has recommenced during the past two years.

Coutts Bank shows a natural Portland face, but the next three or four buildings show the effect of regular washing in the lower parts; here the stone is white and shows signs of natural decay, not however, unpleasantly. The long familiar Caen stone of the Junior Athenæum Club has disappeared during the past two months beneath a cream-coloured wash; it no longer resembles stone. The Green Park Hotel has just been treated by a process which has left it clean and bright, but unlike any Portland Stone. The Scandinavian marble of the Junior Constitutional Club is still disguised by a smear applied some years ago. The Ham Hill stone frontage of the Junior Naval and Military Club has now been made to look like one constructed of cement blocks. Daly's Theatre, faced with the same stone, after several attempts at renovation, was finally painted.

It is clear that in most of these examples there is a gain in our appreciation of the *forms*, but the original *textures* are wholly lost. Both form and texture would be preserved if the buildings were kept clean with plain water—the treatment stipulated by Sir Thomas Brock for the Victoria Memorial. It is dampness and not hard rain that assists the formation of blackness and irregular false shadows in the wrong places.

It will be observed that the most exposed London buildings present the most pleasing appearance: the wind-blown river front of Somerset House, the spires and towers of Wren's Churches, Sir Aston Webb's dome on the Victoria and Albert Museum are all well washed by the rain, and reveal the natural beauty of the stone and the intention of their creators. Compare, on the other hand, the dull and lifeless lower parts and lee sides of so many buildings—St. Paul's, for instance, or the Piccadilly front of the Geological Museum, where the dirt-blackened surface hides the stone, though it may to some extent protect it.

SUMMARY.

We have seen that in the selection of stone many factors have to be taken into consideration over and above those relating to its strength and durability.

Limestones in London now suffer rapid deterioration of their surfaces, which in the present state of the atmosphere is unavoidable unless the stone is protected. The most unsightly defects are caused by local damp conditions, associated in some cases with the action of salts from the mortar or backing.

No protective process has yet been devised that does not require frequent renewal of the treatment. It is conceivable that stone might be impregnated with a strengthening solution, but the cost would be prohibitive and its action doubtful. On the other hand, stone can be protected by the recurrent application of quite a number of materials, including paint; but in the process the valued texture and colour of the stone disappear, so that if this procedure were generally acted upon, there would cease to be the need for special selection of the stone.

What the stone loses in *texture* by being covered with paint or wash, the *forms* given to the structure and its ornament are apt to gain. I think it should be decided in each case which is to be given the preference. If texture and natural appearance of the stone is the end desired then the building should be thoroughly hosed at least once a year, but not more than twice a year. This will not prevent the surface of the stone from slowly receding, but it will maintain a pleasing surface, and the incongruous and often ridiculous patterns and shadows of soot and dirt would not offend the eye. Were this treatment applied to all the limestone frontages in London it would give their form and enrichments their true value, while preventing a good deal of irregular decay, and adding greatly to the light reflected into the street and the interiors of offices, in itself a most desirable object. Similarly, if owners having polished marble or granite façades could be induced to have them wiped down by their window-cleaners at intervals the improvement would be astonishing; both stones and owners would gain thereby. Compare, for instance, the Shap granite shop front at the Piccadilly end of Bond Street with the columns of the Arcade in Albemarle Street.

Under the direction of the Department of Scientific and Industrial Research, efforts are being made at the Building Research Station to grapple with the problems of decay and preservation. Valuable data are being collected and studied, while fundamental matters are being investigated such as the possible effect of injurious bacteria on stone, and the behaviour of stone in relation to changes of moisture and temperature condition. It seems indeed that the recommendations of the Royal Commission of 1861 are at last being carried out.

Meanwhile, the most satisfactory amelioration of stone decay would be attained by the purification of the town air; but great assistance can be given by using stone of higher restivitity, or even some artificial product, in those parts of the building most affected by damp; also by employing ornament either in very bold masses or in low relief, and by designing and constructing with meticulous care to avoid lodgment for damp; and, lastly, by care in the choice and preparation of mortar.

As to the right methods to be employed in the upkeep of historic buildings which have been allowed to become ruinous, I do not venture to speak; in this sphere taste is the arbiter.

I have dwelt on the decay of stone because with the example of the Houses of Parliament before us it is desirable that we should clearly understand what the conditions of decay really are; elaborate Gothic limestones ornament in London spells decay. However, we have by no means done with the use of stone—it is far too convenient and beautiful a material; and whether we approve or not of the modern tendencies in stone construction we may take comfort from the fact that they are at all events giving the stone a better chance, and the pigeons, the dirt, and the damp less foothold.

[J. A. H.]

GENERAL MONTHLY MEETING.

Monday, April 4, 1927.

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

George Beloe Ellis,
Lady Fox,
Walter Heath, M.A. F.R.A.S.
Sir Frederick T. Hopkinson, K.B.E. M.Inst.C.E.
Edward William Mellor, F.R.G.S. F.R.P.S.
Archibald Page, M.I.E.E.
Miss Millicent Somerset,
Sir Josiah C. Stamp, G.B.E. Sc.D. F.B.A.
Arthur Wright, M.I.E.E.•

were elected Members.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., etc., was nominated for Election as Honorary Professor of Natural Philosophy at the next General Meeting on Monday, May 9, 1927, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. F.R.S., etc., was nominated for Election as Professor of Natural Philosophy at the next General Meeting on Monday, May 9, 1927, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

The following Presents received since the last meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Shirley Institute, The Secretary—Shirley Institute Memoirs, Vol. V. 8vo. 1926.

Yale University Library—The Theory of the Gene. By T. H. Morgan. 8vo. 1926. (Silliman Lectures.)

WEEKLY EVENING MEETING,

Friday, April 8, 1927.

Sir Almroth Wright, K.B.E. C.B. M.D. F.R.S.,
Vice-President, in the Chair.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. Pres.R.S. M.R.I.,
Professor of Natural Philosophy R.I., and Cavendish Professor of Experimental Physics, University of Cambridge.

Early Days in Radioactivity.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, April 29, 1927.

Sir Edward Pollock, F.R.C.S., Vice-President, in the Chair.

Edward V. Appleton, M.A. D.Sc., Wheatstone Professor
of Physics University of London, and Fellow of
St. John's College, Cambridge.

Wireless Transmission and the Upper Atmosphere.

It is nowadays becoming more and more evident that the atmosphere has a profound influence on the transmission of electric waves through it, and thus on wireless telegraphy generally. This influence is sometimes favourable and sometimes unfavourable to communication, and the details of some of these effects were considered.

The earliest indication of atmospheric influence was Marconi's successful transmission by spark wireless across the Atlantic in December 1901. The distances previously negotiated by the same investigator were so short as to be explicable on the simple hypothesis that the wireless waves travelled in straight lines. But communication to America, as the late Lord Rayleigh was the first to realise, raised a new question. Could the waves bend round the protuberance of the earth as sound waves bend round a corner? Lord Rayleigh and several other mathematicians investigated the problem theoretically, and their results were unanimous in showing that some influence, other than ordinary diffractive bending, was at work. We now know that the other influence is the so-called Heaviside layer of electricity which is situated in the upper atmosphere, and which guides long wireless waves round the earth's curvature.

The more recent intensive study of wireless transmission, especially on the shorter wave-lengths, has shown that the effects of the Heaviside layer can be detected only a few miles away from the transmitter. As these effects increase with increased distance, they become of importance to broadcast listeners whose receivers are 100 to 150 miles from the transmitting station. The signal fading they experience is due to the arrival of two sets of waves at the receiver, one travelling along the ground and the other from the upper atmosphere after reflection at the Heaviside layer. Many experiments have been made in conjunction with the engineers of the B.B.C. and the National Physical Laboratory, and the height and properties of this layer have been investigated. It has been found to have a height of about 70 kilometres in the day-time. After sunset the height gradually rises, often to a height of 120 kilometres, only to fall to its day-time value on the advent of sunrise. The

Heaviside layer is found to reflect broadcasting waves copiously at night, but in the day-time there is practically no reflection, and only ground waves are received. Listeners situated a long way from the transmitting station, who are only able to receive during the dark hours, often complain of the uncertain intensity and bad quality of their signals, but they should remember that the day-time signal is the one to which they are really entitled. The night-time signal they receive is merely a gift from the upper atmosphere, and they must not complain if the gift is a somewhat variable quantity.

Experiments on the very short waves have shown that the ground waves die out very rapidly, and that reception at great distances is accomplished only by means of waves deflected by the upper atmosphere. But with decreasing wave-length the amount of bending the atmosphere can accomplish becomes less, so that with very short waves a penetration of the Heaviside layer becomes feasible, especially at night when the amount of electricity in the upper atmosphere is least. It has often been said that wireless communication with Mars is impossible, because the Heaviside layer would present an impenetrable barrier to wireless waves, but, from calculations I have made, it appears as if waves of the order of one metre would penetrate it at night, and thus be of no use for long-distance communication. Such wave-lengths would obviously be the ones to use if an attempt to communicate with another planet were seriously contemplated.

A surprising result has recently been found in the effects of magnetic storms on wireless transmission. Such storms are found to interfere very seriously with long-distance short-wave transmission, and yet with very long waves the signals are stronger than usual. It is possible to explain these queer results in terms of the Heaviside layer theory if we assume that a magnetic storm increases the electricity in the layer. In such a case the short waves, which have to penetrate the layer to be bent gradually back to the ground are more strongly absorbed, because they are returned at lower levels where the friction experienced by the electricity is larger. On the other hand, the long waves are truly reflected by the layer at its surface, and an increase in the amount of electricity in the layer increases the amount of this reflection.

The trend of modern wireless development is such that the radio-engineer of the future will aim at using the beneficent influences of the upper atmosphere to the fullest extent. If we attempt to weigh up the advantages and disadvantages of the Heaviside layer, we must count the successes of long-distance transmission, which are brought about solely by atmospheric influences, as more than setting off the fading troubles experienced at various times, so that on the whole the ionised layer must be considered as a real boon to mankind.

[E. V. A.]

ANNUAL MEETING,

Monday, May 2, 1927.

Sir Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S., Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1926, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Six-four new Members were elected in 1926.

Seventy-eight Lectures and Nineteen Friday Evening Discourses were delivered in 1926.

The Books and Pamphlets presented in 1926 amounted to 229 volumes, making, with 569 volumes (including Periodicals bound) purchased by the Managers, a total of 798 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :—

President—The Duke of Northumberland, K.G. C.B.E. M.V.O.
Treasurer—Sir Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S.
Secretary—Sir Robert Robertson, K.B.E. M.A. F.R.S.

Managers.

Sir John Biles, K.C.I.E. LL.D. M.Inst. C.E.
Sidney G. Brown, F.R.S. M.I.E.E.
Sir James Crichton-Browne, M.D. LL.D. F R.S.
J. Mitchell Bruce, C.V.O. M.D. LL.D.
Sir Dugald Clerk, K.B.E. LL.D. F.R.S.
W. H. Eccles, D.Sc. F.R.S.
Viscount Falmouth, L.C.C.
J. S. Highfield, D.Sc. F.R.S.
W. E. Lawson Johnston.
Sir Henry Lyons, D.Sc. F.R.S.
Charles H. Merz, M.I.E.E.
Sir Ernest Moon, K.C.B. K.C. LL.B.
The Hon. Sir Charles A. Parsons, K.C.B. LL.D. F.R.S.
William Stone, M.A. F.R.G.S.
Sir Almroth Wright, K.B.E. C.B. M.D. F.R.S.

Visitors.

James H. Batty.
F. H. Berryman.
W. A. F. Balfour Browne, M.A.
Alfred Carpmael, B.A.
W. Vaux Graham, M.Inst.C.E.
Kenneth R. Hay, O.B.E. M.A. M.B.
Sir Lawrence Jones, Bart. J.P. M.A.
William Macnab, C.B.E. F.I.C.
C. C. Paterson, O.B.E. M.I.E.E.
R. W. Paul, M.I.E.E.
A. O. Rankine, O.B.E. D.Sc.
Hugh Munro Ross, B.A.
W. S. Rowntree, B.Sc.
Sidney Skinner, M.A. F.Inst.P.
F. Coston Taylor, M.A.

WEEKLY EVENING MEETING,

Friday, May 6, 1927.

SIR ALMROTH WRIGHT, K.B.E. C.B. M.D. F.R.S., Vice-President, in the Chair.

MAJOR A. CORBETT-SMITH, R.A. M.A. (Oxon.).

China, and the Real Chinese.

[ABSTRACT.]

THE secret of the Chinese Race, the speaker observed by way of prologue, lay "In the dark backward and abysm of time." To approach that secret it was necessary first to appreciate something of the almost incredible antiquity of the race. In illustration, it was as though one might go to Messrs. Thomas Cook & Son the next morning and purchase travel tickets to view the social life of Rome under Julius Cæsar, or Greece at the time of Pericles. Nearly a thousand years before Moses led Israel out of Egypt the Chinese had an hereditary dynasty. China had witnessed the mighty civilisations of Egypt, of Babylon, of Greece and Rome in their birth, maturity and death. And yet, to-day, one could visit China and behold a social life which, in essentials, was precisely the same as it was when King David reigned at Jerusalem. China was not a country, but a continent. "Marshal all the peoples of the world into fours, and you will find a Chinese in every four."

Suggesting the use of the Magic Carpet, a bird's-eye view was offered of China and its chief physical characteristics. The present political crisis affected but an infinitesimal part of the population; they were in no way concerned with it and cared nothing about it so long as they were left in peace. Eighty per cent. of the Chinese were tillers of the soil, and, of these, the great majority lived upon the hunger line. A farmer with a family of five could live upon from £10 to £15 *a year*, and thought himself passing rich. The average wage in Shansi, the richest province, was about nine shillings a month.

Ancestor worship was the religion of China. That was the secret of the Chinese race. To that basic fact could be traced every phase and feature of the Chinese national and social life. "Understand ancestor worship and you have the key to the Chinese character." A subsidiary creed was that of "fêngshui." That implied some vast, malignant, supernatural force which must constantly be appeased. An example lay in the typical Chinese architecture depicted on the familiar "willow-pattern" plate. Evil spirits could not follow along the little zig-zag bridges. It was "fêngshui" which had prevented the sinking of mines and the development of China's great natural wealth. The sinister grip of that "hidden hand" could be traced everywhere, though the grip was now being steadily loosened.

A review of Chinese characteristics was then given; with the remark that "Unless you approach the Chinese in a spirit of perfect sympathy, as fellow-travellers with us upon our way to the grave, you can never hope to understand and appreciate this mighty and lovable people." Illustrating by examples the invariable cheeriness, good humour, contentment and stoicism which the Chinese set against their lives (to Western ideas) of acute discomfort and often misery he would make special reference to the Chinese national comedy of "Face." That doctrine was of paramount importance to every Chinese, from an emperor in Peking to a beggar under the city wall. A Chinese house-boy would probably walk out of the house, with all the other servants, if his mistress dusted a table. He would have lost "face." At the other end of the scale the existence of a fortified Legation quarter in Peking made for a perpetual loss of national "face." Conversely, the British lost "face" by the recent abortive Hankow Agreement; they were thoroughly despised for it and treated accordingly.

Infinite patience and perseverance, indifference to pain, astonishing vitality and adaptability, with the active converse of an easily roused cruelty, were other marked characteristics. "The Chinese can out-work and under-live every race in the world. Time is meaningless to them. To the Chinese a thousand years are indeed little more than 'an evening gone.' A Chinese will remark, 'If I do not finish this piece of work my son or grandson will.' He will carry a watch, not to tell the time, but because the ticking amuses him so long as he remembers to wind it up." The only form of discomfort a Chinese could not stand was rain. Hence the inevitable umbrella. At Tientsin, during the Boxer rising, hundreds of foreigners had been saved from massacre by a timely downpour. Recently, a Chinese general had broken all etiquette by fighting a battle in the rain. His opponent lodged a strong complaint, with the result that, the next day, the battle was fought all over again, to everybody's satisfaction. But since, in battle, the idea was for both sides to run away and the army which turned round first would win, that incident was readily understandable.

Every nation had its shadows as well as its lights, and the Chinese had vices or disabilities just as the English or French had. Truth and accuracy, for example, like time, were virtually unknown to the Chinese. But, again, a lie was regarded merely as a part of the human comedy. To deceive another was an infinite jest. It was merely a perpetual extension of All Fools' Day. Call upon a Chinese whom you have not previously seen. He may himself receive you and calmly inform you that he is out. To a foreigner nothing was more maddening than the Chinese inability to keep to a point, and their unique talent for misunderstanding, wilfully or otherwise. Their hide-bound conservatism, determined reliance upon primeval methods and customs, immovable conviction that their way was the best way, together with a rock-like passive resistance against any innovation—all these combined to make foreign intercourse extremely difficult.

On the other hand, any business contract upon Western lines was a sheer waste of time. All that a Chinese required was a bond under seal stating broadly that the parties entered into an agreement. No details would be specified. But if any question arose that agreement would be interpreted wholly in a spirit of reason and compromise. Treat a Chinese on those lines and he is your friend. Attempt to bind him by specific conditions and he would beat you every time. The point was of the first importance in the matter of treaties and international relationship.

"Alice Through the Looking-Glass" made an admirable handbook to China, for it was reasonably safe to assert that nearly every Chinese habit and custom was precisely the opposite of Western procedure. By way of example—an Englishman took his dog out for a walk, but dogs being anathema in China your Chinese gives his pet bird an airing. Should your host think that you have overstayed a call he will order a fresh pot of tea for you. A Chinese uses his family or surname in front of his given names. A Chinese visitor keeps his hat on in a friend's house. A foreign lady sews a garment towards her, but a Chinese pins hers to her breast and sews from her. A Chinese book begins at the end and is read backwards from right to left; the footnotes come at the top of the page and the chapter headings down the side. The Chinese magnetic compass points to the south. The foreigner generally builds his house with the more attractive side to the front and throws his refuse out at the back, but the Chinese builds his attractive side to the back and throws his refuse out of the front door into the street.

That last habit suggested the total lack of public and national spirit amongst the Chinese. Local residents would never be induced to co-operate to repair a road frontage. Every householder used to the full the road space before his house for his own affairs, simply because he had nowhere else available. The goodwife will hang out her clothes to dry, the barber will shave his customers, or a family will build a stage for a theatrical performance. When a man and his family lived from day to day upon the borderline of starvation, he was hardly likely to bother about the good of the community. Attempt to discuss with an average intelligent Chinese some public question of the moment, or proposed reform, and he would regard you with astonishment, and ask why ever he should bother his head when the officials concerned paid themselves liberally to attend to it.

All that was the outcome of ancestor worship, as also was the national curse of "squeeze" and avarice. In England tax-collectors and revenue officials were paid government employees. In China a man paid a government representative cash down and a yearly commission for the privilege of tax-collecting. The "tuchuns," or military governors, were concerned solely with making the largest possible sum of money in the shortest time; they then embarked for Japan. Civil war was of the essence of the contract. When the South fell short of funds they applied to the North, who controlled the Customs, for money to keep things going. The request was immediately met.

Women in China were officially non-existent. The root-cause lay in their being debarred from ancestral sacrifice. The old rigid segregation of the sexes was being slowly loosened, but until recent years a brother might not sit at table with a sister over seven years of age; a father might not sit in the same room with a daughter, and male and female garments might not be hung upon the same rack. The handful of shingled and bobbed tub-thumpers, and the so-called "new women" of China were mere local products down the coast-line, and in no sense represented the "real Chinese."

Giving some pictures of family life and customs, births, marriages and deaths, it was remarked that a Chinese was invariably a devoted father to a son, who was regarded as the family's greatest treasure, and, with everything against it, often found genuine love in marriage. But he never wore his heart upon his sleeve. He could even part with his little son to a rapacious creditor with a smile upon his lips and words of exquisite courtesy.

Family privacy was almost unknown. A Chinese would not have the heart to deprive his neighbours of the entertainment of witnessing a family quarrel. And as that party won which could shout the loudest it did not take long to collect an audience. The more relatives and families of one stock that crowd under one roof the better was a Chinese pleased. Millions of Chinese lived in boats upon the waterways. And a typical boat, about as large as the hall of an average English suburban house, would often hold, as a permanent residence, a family of five or six persons.

Some amusing examples were given of the chaos of Chinese currency. The standard unit was the "tael." Of that unit there were some 170 varieties, not one of which existed. For the "tael" was a weight of silver, not a coin. The usual medium was the "cash," a copper coin with a hole in it for stringing numbers together. And since a hundred "cash" were the equivalent of about threepence one would probably need a wheel-barrow to carry the money when going to market. Counterfeit coin were legal tender, but their purchasing power was not so great.

Any form of government "by the people, for the people" was a conception which the Chinese mind could not grasp. It was wholly foreign to the Chinese moral, social and national code. A democracy implied absence of responsibility, and responsibility in one form or another was the foundation upon which the domestic and national life of the Chinese was constructed. Again, ancestor worship was both the origin and the outcome of that responsibility. In every department of Chinese life the links of that chain were visible. The Emperor was the last responsible head. Thus the existing form of republican government had been foredoomed to failure.

From a reference to the lack of internal communications in China, a fact which constituted the most serious obstacle to progress, high praise was given by the speaker to the native genius for agriculture. The Chinese, he said, had a wonderful spiritual affinity with the soil, and in nothing was the genius of the race more apparent. It was one long grim struggle against heavy odds in the stubborn, difficult soil, the spells of drought, the numbing effect of poverty. Yet, ever the cheeriest of souls, looking to the bright side, the Chinese always won through. With a prehistoric hand-plough and hoe he would secure results which a Western farmer with the aid of scientific modern implements could scarcely emulate.

But always, wherever he was set down, a Chinese would swiftly adapt himself to local conditions, from deep-sea fishing in the Arctic to cooking or waiting at table in the Hotel Astoria. And always he would exist and thrive upon the merest pittance of food—a handful of rice, a salted turnip, sweet potatoes or a scrap of fish.

The Chinese were a race of artists, and much of their art and craftsmanship was incomparably beautiful and satisfying. The Japanese had learned all that they knew from the Chinese many centuries ago. The Chinese to-day were as ready as ever to devote years of patient and loving toil to the perfection of a single piece of art-work.

Turning to Chinese architecture, it was observed that even if there were nothing else to see in China yet that wonder of the world, the Great Wall, would compel a visit. One thousand four hundred miles of a primeval stone rampart, it was as fresh to-day over its greater part as in the era of its construction two thousand years ago.

The Drama was the form of recreation easily first in the affections of the Chinese. While the stage was virtually the current literature of the Chinese, yet historical romance was the most popular form. (Ancestor worship again.) Any matter of family rejoicing, a birthday, the winning of a law-suit, a good harvest, was always celebrated by a theatrical performance. And the stage was generally rigged in the middle of the street before the front door. World pioneers in so many things the Chinese must also be accounted the inventors of "programme" music. For in the theatre music played an important part, and by the character of it, the changes of tempo,

&c., the regular theatre-goer always knew precisely what action to expect upon the stage. He could, for example, tell to a nicety whether the village Romeo would be happily united to his Juliet or suffer a lingering death at the hands of the local apothecary. Wagner must indeed look to his laurels.

"What of the future of China and her people?" the speaker said in conclusion. "This great people whose birth is lost in the mists of antiquity, who, standing aloof like the immortals, have watched through the ages the awe-inspiring pageant of world-empires unroll before them and pass, like shadows upon a cinema screen, from darkness into darkness. Again and again through those ages has China suffered the invasion of strange men and strange creeds. Again and again has she experienced not years but centuries of chaos. But time has stood still for her, as it did for Joshua upon Gibeon. Creed upon creed and race after race has China surely and inevitably assimilated. She has built them all, in happy tolerance, within her own social structure. And, in so building, she has given constant and indisputable proof of the value of her own national creed, which is profound respect for the individual and for his own personal happiness.

"The Chinese hold firmly to the conviction that their civilisation is the highest in the world. If civilisation implies an instinctive and personal happiness, then I confess myself in agreement with the Chinese. The average Chinese is infinitely happier than the average Englishman. For we, I think, have lost the art of true happiness. We lost it with the coming of the Industrial Revolution and the impact of modern science.

"China, in turn, now confronts that same impact. Will science conquer the Chinese or will the Chinese conquer science? Are the materialism and so-called 'progress' of Western civilisation to engulf these haunts of ancient peace, of kindly sentiment and courtesy, of artistic temperament and practical wisdom, whose people seek only leave to work out their destiny in their own unobtrusive way? Or will the Chinese, with their innate passive resistance and adaptability, seize and incorporate this invader too within themselves? Will they reject the unworthy, and, upon all that is of good report, both of their own humanity and of Western science, re-create a society which shall live into the future as it extends back into the past?

"The longer and the better a man knows the real Chinese the deeper becomes his admiration, respect and affection for them. 'Justice and right thinking,' said Confucius, 'must ultimately conquer might.' I believe that when England also has lived her little day and passed with Greece and Rome and the kingdoms of the West into a golden memory, this 'Flower' of the Middle Kingdom will have come to blossom and that her azalea petals will bespread the world."

[C.-S.]

GENERAL MONTHLY MEETING.

Monday, May 9, 1927.

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Chairman announced that the President, His Grace the Duke of Northumberland, had nominated the following Gentlemen as Vice-Presidents for the ensuing year:—

Sir James Crichton-Browne, M.D. LL.D. F.R.S.
J. Mitchell Bruce, C.V.O. M.D. LL.D.
Sir Dugald Clerk, K.B.E. LL.D. F.R.S.
Sir Ernest Moon, K.C.B. K.C. LL.B.
Hon. Sir Charles Parsons, K.C.B. LL.D. F.R.S.
Sir Almroth Wright, K.B.E. C.B. M.D. F.R.S.
Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S., Treasurer.
Sir Robert Robertson, K.B.E. M.A. F.R.S., Secretary.

Mrs. Madge Smith Atkinson Adam,
Daniel Kenneth Capper Birt, LL.B.
Sir Arthur McDougal Duckham, K.C.B. M.Inst.C.E.
Philip Vassar Hunter, C.B.E. M.Inst.E.E.
Samuel Insull, D.Sc.
Archibald McKinistry,
Charles P. Sparks, C.B.E. M.Inst.C.E.
Sir Alexander Walker, K.B.E.

were elected Members.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., etc., was elected Honorary Professor of Natural Philosophy, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. F.R.S., etc., was elected Professor of Natural Philosophy, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

The following Presents received since the last meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

Belaiew, Colonel N. T., C.B. M.R.I. (*the Author*)—The History and Development of Russian Weights and Measures. 8vo. 1927.

General Society of Mechanics of the City of New York—Library Classification and Numbering System. By H. W. Parker. 8vo. 1926.

Hadfield, Sir Robert, Bart., D.Sc. F.R.S. M.R.I. (*the Author*)—The Metal Manganese and its Properties. 8vo. 1927.

Alloys of Iron and Manganese containing Low Carbons. 8vo. 1927.

Hill, W. Leonard (*the Publisher*)—Chemical Engineering and Chemical Catalogue. 4to. 1927.

Jamieson, Thomas, Esq. (*the Author*), *Executors of the late*—Nature's Serious Tale. 8vo. 1926.

WEEKLY EVENING MEETING,

Friday, May 13, 1927.

Sir James Crichton-Browne, M.D. LL.D. F.R.S.,
Vice-President, in the Chair.

Sir Humphry Rolleston, Bart., K.C.B., M.D., LL.D., D.Sc.,
Regius Professor of Physic, University of Cambridge.

Concerning Old Age.

In considering old age, of which it is to be hoped we shall all have a happy experience, for there is a normal and cheerful as well as a morbid, crabbed, and unhappy form, it is hardly possible to avoid a few words on the duration of life. In the simplest forms of animal life—the protozoa—which consist of a single cell, and so stand in the same relation to man that a brick does to a city, multiplication occurs by fission or division into two halves, and as there are now two cells instead of one and no vestige of a corpse, the organism is, as Weissmann first insisted, immortal. This process may go on indefinitely, and has been watched by Woodruff in paramecium, during 13½ years, for 8,500 generations, comparable to a quarter of a million years of human life, without the occurrence of conjugation—namely, the union of two previously separate organisms, as occurs in the reproduction of higher animals—though periodically rejuvenation appears to take place by means of an independent internal re-organisation (endomixis) of a single cell. As the protozoa are, accidents apart, immortal, why is it that animals much higher in the scale never are? It has been shown by culture experiments that the individual cells of man's body are also potentially immortal, but that the necessary conditions for this cannot be realised when they form part of a highly differentiated and specialised complex whole. In the higher walks of the animal kingdom rejuvenation of the constituent cells is thus rendered much more difficult or impossible, and the process of senescence must be regarded as the penalty for the high degree of individuation entailed in the complicated mechanism of the higher animals and man. But it is interesting to recall that in the humble planarian flat worms the life cycle may be dramatically changed: by starvation they not only become smaller, but their structure becomes simpler, and their life cycle is reversed by the process of de-differentiation, reduction or involution. By alternate starving and feeding these flat worms have been kept stationary, while others passed through nineteen generations (C. M. Child), thus showing that the duration of existence of cells is not so much a matter of time as of the work or metabolic changes that go on inside them, although it may well be said that such a prolonged existence is not "life" according to some views; and although it is a far cry from amoebæ and flat worms to man, these biological results may have some remote bearing on the conditions influencing the duration of man's life. Not only diet—over- and under-feeding, the effects of gross feeding being obviously harmful—but the more mysterious influence of the internal secretions provided by the ductless or endocrine glands, the thyroid, pituitary, and the gonads, the activities of which may well be modified by diet, must be taken into account. Disturbance of the endocrine balance, or the equilibrium normally maintained between the hormones or chemical messengers of these endocrine glands, has well-recognised effects on growth and metabolism, and so may influence the rate at which the body lives and wears or rusts out. The significance of the much-discussed "rejuvenation" produced by Steinach's and Voronoff's operations on the sexual organs prove at any rate that there is much to learn, especially whether or not the duration of life is really prolonged thereby.

BIBLICAL AGES.

The length of days ascribed to Methuselah (969), to Adam (930, or if the "conceit urged by learned men," as Sir Thomas Browne says, that he was 50 or 60 years old when called into being be accepted, making him 980 years at death), to Jared (962), to Seth (912), and others, has of course always excited curiosity. In *Arbor Vitae or a Physical Account of the Tree of Life in the Garden of Eden*, "translated out of the Latine of E. M. Arrais, M.D., Physician to John the IV King of Portugal, by Richard Browne in 1683," there is a discussion as to the influence of the emanations of the Tree of Life, the eating of which and the resulting immortality were prevented by cherubims and a flaming sword turning every way, in endowing our first parents and their descendants with longevity, and the conclusion is reached that their length of life was due to the virtues diffused in the air, not only of Eden but of the neighbouring countries, given off by fruit trees other than the Tree of Life in the Garden of Eden. The longevity of the patriarchs has called forth critical attempts at explanations on the basis of a difference in chronology. But though interesting and perhaps ingenious they are not very convincing; the suggestion that the reputed "years" were lunar not solar, containing thirty and not three hundred and sixty-five days, is too radical, for some of the patriarchs would on this interpretation have begotten children before they were ten years of age. The view that the year consisted of three instead of twelve months up to the time of Abraham, when it was extended up to eight months until the time of Joseph, when it first became our full complement of twelve months, would make Methuselah's age 243 years, which is still excessive by modern standards. The probable explanation is that the patriarchal ages, like those of the mythical golden age of pre-history, existed solely in the minds of subsequent scribes only too anxious to magnify the fathers of mankind. Antediluvian chronology may perhaps be compared with that of the Puranas, or the common scriptures of the ruling Aryan peoples of Northern and Western India, which contain fragments of truth and much that is imaginary, including in the last category the chronology (Rapson*).

The natural or physiological life of man has been thought to be a multiple of the period of growth. Hufeland, taking twenty-five years as the end of adolescence, and accepting Francis Bacon's view that animals live eight times as long as they take to come to maturity, optimistically arrived at two hundred years as the span of man's days on earth ; Buffon multiplied fourteen, the age of puberty, by seven, and so concluded that a hundred years was the appointed time. Flourens, on rather different grounds, came to the same estimate, as did the often-quoted centenarian Luigi Cornaro, of Venice (1467–1566), and in more modern times Metchnikoff and Luciani. The number of reputed centenarians, especially in Russia (1 in 1,000), Bulgaria (1 in 2,000), California (1 in 10,000), and in Ireland (1 in 14,300), is at first sight imposing, but has been sternly discounted and cut down by George Cornewall Lewis, W. J. Thoms, and especially by T. E. Young's accurate investigations, and shown, contrary to the general view and other statistics, to be extremely small. Thus we must regard as fabulous the famous examples of Henry Jenkins, Thomas Parr, Katherine, Countess of Desmond, and probably other records, such as the Indian, quoted by Arrais on the authority of many Portuguese Indian Governors, who lived more than 335 years, and the truthful tombstone in Carmarthen recording the death in 1831 of Ann David, aged 181 years.

Here are a few lantern slides of well-known, if not well-authenticated, centenarians :—

Henry Jenkins, aged 169 years (1501–1670), who remembered picking up arrows at the battle of Flodden Field in 1513 when twelve years old.

"Old Parr," Thomas Parr, aged 152 years (1483–1635), of Winnington, Shropshire, who first married at the age of 88, had some love complication and had to do penance in a white sheet when 105, and married again, a widow, when 122. In 1635 he was brought up to London to be presented to Charles I., but his hospitable welcome proved to be his undoing, for staying as a servant in the house of the Earl of Arundel he had the opportunity of drinking wine and died. William Harvey, the discoverer of the circulation, who did Parr the honour of performing the postmortem examination, found that his organs were remarkably healthy, and ascribed death to a mere plethora brought on by the more luxurious living in London than he had been accustomed to in his home, where his food was very plain. By his first wife he had two children who died young, but rumour gave him five centenarian descendants.

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* Rapson, E. J., in "The Cambridge History of India," 1922, vol. i. p. 305.

Katherine, Countess of Desmond (1464–1604), of Inchiquin, Munster, was reputed to have lived 140 years, to have had a daughter after she was 65 years of age, and a second set of natural teeth shortly before her death. The critical W. J. Thoms considered that the lives of three different countesses were united into one in her.

Petratsch Zortan (1537–1724), a Hungarian, said to have lived 187 years, is here represented as he was two years before his decease.

A Hungarian couple, John Roven and his wife Sarah, at their reputed ages of 172 and 164 years, whose married life lasted 147 years.

An English couple, Benjamin and Elizabeth Atkins, of Brockdish, centenarians of a more modest but more reliable kind, as they were investigated by the late Sir George Humphry.

Here is a genuine old man of fifteen years in his clothes, though he shows better without them, for "the clothes make the boy." He is an example of progeria, fortunately a very rare condition, described by Mr. Hastings Gilford, from whom this photograph is taken, in which premature senility early supervenes on infantilism, so that the greater part of life is cut out. It is possible that the gentleman in W. S. Gilbert's *Bab Ballads* "who died an enfeebled old dotard at five" was another example.

Fallacies easily creep in, as is so clearly explained by Raymond Pearl ("The Biology of Death," p. 23, 1922):—"John Smith was born in the latter half of the eighteenth century. His baptism was duly and properly registered. He unfortunately died at the age of say 15. By an oversight his death was not registered. In the same year that he died, another male child was born to the same parents, and given the name of John Smith, in commemoration perhaps of his deceased brother. This second John Smith was never baptized. He attained the age of 85 years, and then because of the appearance of extreme senility which he presented, his stated age increased by leaps and bounds. A study of the baptismal records of the town disclosed the apparent fact that he was just 100 years old. The case goes out to the public as an unusually well-authenticated case of centenarianism, when of course it is nothing of the sort." Further, the really old take a not unnatural pride in their power of survival, and tend to magnify their prowess; thus, according to W. G. King, writing in 1911, the census showed a larger number of persons over 91 than between 85 and 90 years of age.

Since the middle of the last century the great reforms in hygiene and sanitation in this country prevented illness and premature death to such an extent that the expectation of life at birth has increased from 39·91 years in 1854 to 51·5 years in 1912 for males, and from 41·9 to 55·35 years for females. These official figures have kindly been given me by Dr. T. H. C. Stevenson, of the General Register Office, who tells me that provisional rough tables based on the 1921 census and recently published, but not official, show a further increased expectation of life. This increased expectation of life at birth is due to the prevention of death in early life, and is therefore very different from an increase in longevity. In fact the statistical conclusion from investigations of the ages recorded on mummies in Egypt two thousand years ago by Karl Pearson, and others quoted by R. Pearl, is that the expectation of life at advanced periods has declined. Karl Pearson in 1902 found that, though a man of twenty-five lives on an average fifteen years more than one two thousand years ago, the expectation of life after the age of sixty-eight for a Romano-Egyptian two thousand years ago was greater than for an English man or woman of the same age, and pointed out that with these ancient people at about the beginning of the Christian era it was a case of the survival of the fittest, whereas the difference in the expectation of life at earlier ages is evidence of the great social and sanitary progress that has conquered environment.

While the Psalmist's estimate that "the days of our age are three score years and ten; and though men be so strong that they come to four score years, yet is their strength then but labour and sorrow" is still the common experience; there are of course exceptions that we can all point to. In his prize essay on comparative longevity written in 1870, Sir Ray Lankester compared persons of abnormal longevity with giants in height which might range up to nine feet; but it is now known that these giants are morbid and examples of acromegaly, described by Pierre Marie in 1886, and it would be curious seriously to regard extreme longevity as a disease, as it is so obviously favoured by absence of that state. On balance it may be still thought that the physiological age of man is a hundred years, though few there be that reach it. Women live longer than men; and among reputed centenarians also the ratio is much in favour of that popularly said to be the weaker sex; out of 691 reputed centenarian deaths during the ten years 1910–19 inclusive, 504, or 73 per cent., were females, and 187, or 27 per cent., males. In 1923 the numbers were 74 females and 22 males.

The reason for the limitation of life, in other words death, has naturally been much debated. Weissmann suggested the thesis—a perverse extension of the theory of natural selection and survival of the fittest—that death was an adaptation advantageous to the race. Death, which has thus been evolved in the process of advance from a protozoan to the higher metozoan standard of existence, might be regarded as a provision against over-population and famine from insufficiency of the food supplies of the world, and so the natural counterpart to modern artificial birth control. Miles Symner, Professor of Mathematics in Trinity College, Dublin, in the 17th century, calculated that in the 1400 years between the Creation and the Flood there would not have been standing room on the earth's surface if the Patriarchs with an average breeding period of four hundred years begat a son every three years. With the present rapid increase of the world's population, thanks to improved sanitation, there have not been wanting estimations that, should no change occur, a few centuries will show the food supply of the world inadequate for its inhabitants (R. Pearl; Ravenel).* Metchnikoff considered death to be the result of intoxication from bacterial activity in the alimentary canal, and it has by many been ascribed to arterial disease; the latter is no doubt a common cause of death, but not of the physiological termination of life. But a more satisfactory conception is an inherent constitution determined by heredity, the constituent cells being thus endowed with a certain store of vitality for themselves and their descendants, and that as this runs out the process of involution begins. Cell culture observations, however, suggest that there gradually develops in the cells a substance which inhibits their vitality. Carrel and Ebeling† found in cultures of fibroblasts the rate of multiplication and life *in vitro* varied in inverse ratio to the age of the fowl from which the blood plasma, used in the culture, was taken. It was also found that with frequent washings to remove waste products tissue cells can be cultivated indefinitely for years *in vitro* and have an unlimited capacity for multiplication (Carrel, Champy and Grandcourt). It would thus appear that there is in old organisms a substance produced in the ageing cells which, entering the blood, exerts an inhibitory action on the life and reproductive power of the tissue cells. On the other hand, Raymond Pearl avoids the assumption of an inhibitory senescent substance in the blood plasma by the simple suggestion that the blood plasma of old animals is itself senescent, and so not such a good culture medium as the blood plasma of the young.

FACTORS INFLUENCING LONGEVITY.

Experience and that pure and reformed summary of it known as statistics show, as Karl Pearson and A. Graham Bell's figures prove, that inheritance is one of the strongest factors, if not the dominant one, in determining the span of life. The other factors are extrinsic and may be included under the heading of environment in its broadest sense, if we may now use a word which in 1835 John Sterling, in reproaching Carlyle for so doing, described as "barbarous" and "without authority."

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* Ravenel, M. P., The Gordon Bell Memorial Lecture, 1925, on "The Prolongation of Life. To what Goal is it Tending?" *Am. Journ. Public Health*, New York, xvi. supplement to February number.

† Carrel and Ebeling, *Journ. Exper. Med.*, Baltimore, 1921, xxxiv. p. 599.

Numerous examples of families with long or with short-lived tendencies will occur to everyone; but what heredity can do is perhaps more convincingly established by Raymond Pearl's striking table, from Bell's analysis of the Hyde family, showing the influence of parental ages on that of the offspring: among 184 persons whose parents both lived more than 80 years the average age at death was 52·7 years, whereas among 128 persons whose parents died before 60 years of age their average life was 32·8 years, or nearly 20 years less. Forsyth's investigation, carried out on strict actuarial principles, shows that if the reasonably preventable diseases be eliminated the expectation of life, though increased, would not be so much prolonged as by the influence of a long-lived heritage. We should, therefore, both in our own and their interests, counsel our children to choose their parents carefully. As Benjamin Ward Richardson pointed out, the combined ages of one's parents and grandparents divided by six may be of assistance in numbering our days.

A sound stock may overcome the evil influence of environment, such as alcoholism and unhealthy surroundings in towns, and thus explain the occasional longevity of those whose lives have been far from blameless, and the contrast between two aged brothers, one sober, the other intemperate. But even with a poor inheritance care may extend the term of a useful and happy life. Sir Hermann Weber, by practising the precepts of his book on *The Prolongation of Life*, led an active and happy existence until within a short time of his death at 95, although his parents died at or before 60 of heart failure and cerebral hæmorrhage—conditions due to high blood pressure and arterio-sclerosis.

Of the hereditary factors inherent vitality of the nervous and the vascular systems are the most important; the force of the nervous tissues calls the tune both in the mental disposition and the physical reactions of the body. As the late Sir William Osler remarked, much depends "on the quality of arterial tissue (vital rubber) which the individual has inherited." This statement, it may incidentally be mentioned, provides the answer to the question, "What internal evidence is there that Osler has had an unhappy experience with cheap bicycles?" set in an examination paper, analogous to Calverley's famous examination paper on "The Posthumous Papers of the Pickwick Club," on his text-book of medicine. Cazalis' aphorism "Man is as old as his arteries" is true in so far as the state of the vessels is in certain respects a good index of his prospects, for their condition may be largely determined by heredity, or deteriorated by disease or undue strain, and so provides a record of his family history and personal adventures; but the converse, "the arteries are as old as the man" would not hold good, for arterio-sclerosis is not necessarily well marked in very old people.

Bodily Conformation.—The long-lived are usually spare and very seldom fat, neither very tall nor very short, very heavy nor very light. Insurance companies are naturally much interested in the bearing of the body-build on longevity, and in America much statistical investigation has been started into problems such as the bearing in persons above the average weight, of the length of the spine, and a comparison of the chest and abdominal girths. From the insurance companies' statistics Dublin* found the following rather surprising and confusing results:—Among those over the average weight the prospect of life was better among short and medium height men with short relative spine-lengths and with chest girths below the average, and among overweight tall men the outlook was best in those with long relative spine-lengths and chest girths below the average. So that in both of these groups of men above the average weight the presence of a chest measurement below the average was an asset.

Taking *environment* in its widest sense, the influence of disease in damaging the tissue cells and in accelerating degeneration and atrophic processes resembling those which normally occur must be given due weight. It has been said that only those who have kept their bodies free from disease up to the age of 60 can expect to attain extreme old age (Saundby); but nearly half of the 824 persons between the ages of 80 and 100, analyzed by Sir George Humphry, had passed through a severe illness, many of them acute infections. It seems highly probable, however, that the influence of an acute illness, the resulting changes being often transient and recoverable, would be much less harmful than that of a long continued infection or intoxication in producing permanent changes. There are exceptions, and a terribly notable one is encephalitis epidemica, the "sleepy sickness" of the lay press. Chronic infections, especially those in connection with the teeth, are prone to cause rheumatoid arthritis, and the resulting limitation of activity does harm both to the general health and mentally by impressing the idea of future crippling and incapacity. Acute illness may undoubtedly be the apparent starting point of old age, more particularly if insufficient time, holiday and change of scene be not allowed for recovery, the time allotted increasing roughly with the nature of the illness and the patient's age. The influence of infections, such as malaria, which Dr. W. H. S. Jones has shown was an important factor in the decadence of Magna Graecia, and of syphilis, both on the collective and individual health and longevity need not be laboured.

The ideal of medicine is the prevention rather than the cure of disease, and for this end the detection of the earliest stages, and better of the disposing causes of diseases, is essential. Timely warning about diet, exercise, and manner of life may do much to prevent disease from getting a firm seat on a man's back, and it is not without significance that life assurance companies in America have found it pay to provide periodic medical overhauls to their clients.

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* Dublin, L. I., De Lamar Lectures, 1925–1926, p. 113.

Functional activity, mental and physical, plays a great part in keeping the body, when free from disease, trim and slim and in postponing the advent of morbid old age. Occupation with a keen desire to carry it through is so beneficial that some, such as Karl Marx, would regard old age as largely a question of will power. Speaking of the circle in which Madame du Deffand moved, Lytton Strachey * says: "They refused to grow old; they almost refused to die. Time himself seems to have joined their circle, to have been infected with their politeness, and to have absolved them, to the furthest possible point, from the operation of his laws. Voltaire, d'Argental, Moncrif, Henault, Madame d'Egmont, Madame du Deffand herself, all lived to be well over eighty, with the full zest of their activities unimpaired."

Retirement is a problem beset with anxiety and danger; a successful business man when relieved of routine and able to indulge in idle luxury and without hobbies may rapidly degenerate. He now has to find occupation to kill time instead of time to do all he must; he begins to feel that "his day is done," that he has little but his dinner to look forward to, and that at last he is old and a "has been." Thus auto-suggestion, even if not helped by suggestion from outside, hurries him on the downward path; any trifling ailment, such as rheumatism or indigestion due to overfeeding, may arouse very hypochondriacal anticipations, and observation of contemporaries with premature senile changes may feed the flame of this destructive auto-suggestion. There is, therefore, a basis for the idea that senility is catching, and for seeking the companionship of the young and thereby letting auto-suggestion work in a constructive direction. A well-occupied mind, a happy disposition that thinketh no evil, naturally smiles instead of frowning on a stranger or a new idea, free from anger, hatred, and jealousy, the vice that gives no pleasure to anyone, and an attitude of charity in its original and best sense to all, tend to prolong life and make it a happy, healthy prelude to crossing the bar. In Sir James Crichton-Browne's words, "the best antidote against senile decay is an active interest in human affairs, and those keep young longest who love most."

As showing the influence of long-continued active work reference may be made to the longevity of dignitaries of the church and the bench, and prime ministers, though these are no doubt supermen. Among painters there are some examples of very long lives fully occupied up to almost the end—Giovanni Bellini, Michael Angelo, Sidney Cooper, Luke Fildes: Titian, for example, was painting with "incomparable steadiness of hand" when cut off by the plague at ninety-nine. Public duties may provide a means of useful and health-maintaining activity; thus during recent years there have been two octogenarian Lord Mayors of London, Sir Thomas Crosby (1911) and Sir John James Baddeley (1921). Compulsory retirement on attaining an age limit in the civil and military services, and now at the older universities, may not always be in the best interests of the individual or of the community; but as the majority of septuagenarians suffer in greater or less degree from pathological old age it is a good general rule; and those who fall under the axe may comfort themselves on escaping the danger of epitaphs, which add another terror to death, like that bestowed in the *Reminiscences of the University, Town, and County of Cambridge*, begun by Henry Gunning when he was over eighty, to the effect that "Professor Edward Christian died in 1823 in the full vigour of his incapacity."

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* "Books and Characters," p. 83, London, 1922.

Food.—Experience, ancient and modern, both lay, such as is set forth in Cornaro's *Discourses on a Sober and Temperate Life* (1558), and medical, as contained in the works of the great George Cheyne (who at one time weighed 32 stone before he became a vegetarian), author of *An Essay of Health and Long Life* (1724), Metchnikoff, Henry Thompson the surgeon, and Hermann Weber the physician, agree on the importance of moderation in food. Most centenarians have been small eaters, especially of meat, and there is this advantage in poverty and living on Abernethy's earned sixpence a day, or its present-day equivalent. Excess of food, though more gradual in its evil influence, is more generally destructive than alcoholism, and there is much wisdom in Montaigne's dictum, "Man does not die, he kills himself," and in the more dramatic proverb, "You dig your grave with your teeth." As already mentioned, in some lowly forms of life, such as planarian worms, partial starvation leads to rejuvenescence and prolongs life, and there have not been wanting "cures" on such economical lines. A simple diet throughout life, and, after growth is completed, obedience to the rule of ceasing to eat before a feeling of repletion cries, "Hold, enough," can be confidently recommended.

The influence of alcoholic drinks has been much discussed, and here, as in most problems, the influence of personal predilections may unconsciously tinge our conclusions. But there seems no doubt from collective investigations, such as Sir George Humphry inspired, and insurance companies provide, that hard drinkers are exceptional among the long-lived, and that among the characteristics of those who attain great length of days temperance finds a place. But in spite of the opinion of some enthusiasts that the choice of total abstinence is the only sure path to longevity, the recent investigation, undertaken with all a modern statistician's precautions against fallacies, by Professor Raymond Pearl * on over 5,000 individuals at

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* Pearl, R., "Alcohol and Longevity," 1926; and Leading Article, *Brit. Med. Journ.*, 1927, i. 528.

Baltimore gives the perhaps not unwelcome verdict that "a moderate use of alcohol does not tend to shorten life." Such an opinion, based on sound data, is of unquestionable value; and this I gladly admit, for my impression was that long-continued and constant absorption of alcoholic drinks, though always moderate and never amounting to intoxication, did tend to age the conscientious devotee; but a few positive instances are apt to make an undue impression. What constitutes a "moderate use" is open to the criticism brought by Sir Hermann Weber,[*] that what may be so for one is excess for another; and the proverb, "Wine is the milk of old people" has been countered, as is the habit of proverb quoters, with "*Vinum lac veneris*," and the danger of exposure to other risks. In forming a conclusion in any given case about the question, to drink or not to drink, the personal equation of the individual must weigh heavily. No doubt total abstinence may suit some better and some worse, and while it certainly eliminates the problem of what is moderate it equally deprives the aged of the help and comfort that some undoubtedly thus receive.

Smoking is on rather a different footing from excessive eating and drinking; as Calverley said, "Stories, I know, are told not to thy credit," but whatever may be true of the effects of the injection of nicotine into cats and rabbits, tobacco smoking has not been proved to cause arterio-sclerosis in man. As Sir Clifford Allbutt, a non-smoker and with a peculiar susceptibility to tobacco smoke, pointed out, if in any way it does cause arterial disease the process is so slow, at any rate in most people, that its effects become so mingled with other manifestations of old age as to be almost impossible to discriminate: further, as Ruffer[†] proved by examination of mummies, the ancient Egyptians (1580 B.C.–525 A.D.) certainly had arterio-sclerosis without the consolations of tobacco. There is no doubt that the tolerance, painfully acquired in youth, to tobacco commonly diminishes with advancing years, and that unpleasant symptoms ranging from irregularity of the heart, abdominal pain to tobacco angina may be the means of transforming a previously inveterate smoker into a total abstainer. Sir George Humphry,[‡] also a non-smoker, specially investigated the habits in this respect of centenarians; among 19 men 8 smoked much, 1 a little, 10 not at all; of 30 female centenarians 4 smoked much, 2 moderately, and 24 not at all.

There are statistics to show that people in the country live longer than those in towns.

The advice then to give others and even to practise ourselves should include a judicious choice of parents, avoidance of disease and worry, moderation in all things, mental and physical exercise, an open-air life, serenity and charity to all men. Leonard Williams's epigrammatic summary is easier to remember: "Fresh air, meagre food, freedom from care."

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[*] Weber, Hermann, "On Means for the Prolongation of Life," p. 119, 1908.
[†] Ruffer, M.A., *Journ. Path. and Bacteriol.*, Cambridge, 1911, xv. 453.
[‡] Humphry, G. M., "Old Age," pp. 58, 63.

The onset of what is popularly called old age varies in different countries: for example, the wheels of life run much faster in tropical countries, such as India, than in temperate latitudes. In the same race there is considerable variation: one man at sixty is prematurely senile—the subject of pathological old age—whereas another aged eighty is vigorous in body and alert in mind. In women the menopause is a milestone, and at any rate is the frontier of the territory of old age. There has been said to be a similar climacteric in man at fifty or sixty, due to changes in the reproductive organs, but without, I believe, any solid foundation; it may be a survival of the ancient conception of the grand climacterics at 49 (a multiple of the number 7), at 63 (7 multiplied by the magical number 9 of the Arabians), and at 81. Sir Henry Holland in 1873 described the climacteric disease, which admittedly seldom came on without some previous event, such as cold, gout, a bout of drinking, recent marriage, or bereavement. There can be no possible shadow of doubt that after an illness recovery becomes slower as the years roll by, and that often, especially when the proper period of convalescent repose is abbreviated, the onset of old age is first noticed after an illness; the moral is to hurry slowly after illness, be it influenza, operation, or worse. A senile climacteric about the latter part of the seventh or eighth decade in man, marking the dividing line between old age and decrepitude, as drawn in the twelfth chapter of Ecclesiastes, has been described by Nascher, but it cannot be regarded as comparable to the menopause in the other sex. To put the beginning of old age at fifty or even sixty in man would no doubt raise the protest that it is only the elastic period of middle age. In truth there is so much variation that a rigid date cannot be fixed. In healthy people the advent of old age is so gradual that the individual himself has no suspicion of it, and very likely secretly preens himself on looking ten years younger than his years and his contemporaries, whose changed appearance arouses self-congratulation rather than self-examination. Perhaps the suspicion suddenly breaks upon him by overhearing a chance remark of others, by seeing an unwonted reflection of his figure in a mirror, or by some nice girl offering him her seat in an omnibus. Or a holiday may break to him that he cannot walk as of yore because of undue fatigue, or he is held up by shortness of breath or pain on unwonted exertion.

THE PHYSIOLOGY OF OLD AGE.

While the whole body does not age at the same rate, there is a general and progressive diminution in functional activity corresponding to the atrophic involution of the cells of the organs and tissues.

The response to stimuli of all sorts is diminished, and this sluggish reaction contrasts with the ready and comparatively exaggerated response to both normal and pathological stimuli in early life. The popular opinion that age is second childhood may be correct in that there is some resemblance between undeveloped and failing mental and physical powers, but there is an enormous difference between the ever actively moving child and the impassiveness of real old age. This failure of the power of reacting to stimuli is seen in the sense organs; few people over sixty have perfect hearing, although most of us do not know it; and apart from loss of acuity they often do not take in general conversation so well as others unless their attention is braced up for the purpose. The emotions are less active, the death of friends is less of a grief, and so the individual tends to become isolated and to live more on past than on present-day impressions; hence a well-educated man may become more composed and satisfied, whereas one without intellectual interests may sink into mental torpor, vanity, and egocentricity, with the development of fads about health, undue garrulity, and a confirmed attitude of the *laudator temporis acti.* In what may be regarded as normal old age psychical activity wanes; new ideas do not bubble up, and when brought to his notice are not impetuously accepted. On the other hand, there is more of the philosophic calm born of what, when twitted with impatience, the young university don dismissed as "that greatly overrated property experience." Forgetfulness first of names, and much later of recent events, and mental fatigue are other evidences of the change. With loss of memory comes the habit of repeating the same story or remark, of mislaying things, and of becoming careless about the external graces.

Statements that the race is all to "the younger generation," and that forty marks the end of useful work in the world, are of course exaggerations, and many examples could be brought forward of masterpieces in art, literature, and science which have come from men much above that age. But taking the average it must be admitted that the elasticity, imaginative power, and originality of mind that produce works of genius and great advances are seldom found in those who have two score years and ten to their credit. In the early stage when a suspicion of the approach of age begins to dawn there may be defensive action in the apeing of the young, so as to conceal the true age: a man may withdraw the date of his birth from "Who's Who" and other books of reference, and a mother has been known to delay the coming out of her daughter.

While calmer and of more mature judgment as a rule, many, but perhaps not quite normal, old people become more anxious and apprehensive; like their gait, their will power is hesitant. In old animals the instinct of self-preservation seems to fade, and as their time draws nigh have perhaps, if we could only know, a desire for death, comparable to that for "his brother Sleep"; old people, though they often complain of their state, generally have what

Matthew Arnold called "passionate, absorbing, almost blood-thirsty clinging to life." This, however, is not universal, and shortly before the end often disappears so that death is regarded as a welcome release, as is borne out by the famous William Hunter's last words when only sixty-five, "If I had strength enough to hold a pen, I would write how easy and pleasant a thing it is to die."

Muscular power and ability to walk as far and as fast as of yore diminish, the muscles and the glands of the intestine become sluggish so that constipation results, the diminished secretion of cutaneous perspiration makes the skin dry, and the bodily exchanges, as shown by basal metabolism estimations, are less (Aub and Dubois*; Legrand†). But except that the temperature in the axilla is lower than normal on account of the diminished blood supply to the skin, the internal temperature of the body is not, as might be expected, and indeed has been stated, lowered. The explanation appears to be that although there is less heat produced in the body there is less loss of heat from the dry skin on account of its poor vascular supply. Diminished sensibility to pain, both mental and bodily, is a beneficial relief, and suggests that with the gradual involution and approach to physiological death this warning will no longer be needed. This in some degree accounts for the latency of disease, such as pneumonia, urinary calculi, gallstones, or cancer, sometimes shown by the aged. There are, however, exceptions such as pain after zona (shingles) and obstinate itching. Sleep is less continuous than in youth, and early waking is common. Recovery from illness is slower, wounds and fractures of bones take longer to heal, and intracapsular fracture of the neck of the femur, which is prone to occur from the thinning of the bone and the altered angle at the junction of the shaft and the neck of the bone, is a serious accident, often followed, as if it were the last straw, by dissolution. The old react somewhat differently to drugs, responding less promptly, so that some, such as purgatives, may be needed in considerable quantities. On the other hand tolerance, as to tobacco, may become so much diminished that idiosyncrasies to certain drugs may appear; morphine should be preceded by a previous dose of atropine to protect the senescent respiratory centre from being put to sleep, but the old are hardly so susceptible as children to this drug.

Though a gradually rising blood pressure is commonly seen to accompany the passing years, a high blood pressure is not a feature of old age; in fact it might be said that those who attain great length of days do so, in part at least, because they have not had a high blood pressure to wear out and thicken their blood vessels. The pulse often shows intermittence (extrasystoles); this was so in one-fifth of the 824 persons analyzed by Sir George Humphry, and though sometimes a cause of annoyance is not of any import.

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* Aub and Dubois, *Arch. Int. Med.*, Chicago, 1917, xix. 823.

† Legrand, R., *Rev. franç. de l'encrinolog.*, Paris, 1926, iv. 199.

NORMAL STRUCTURAL CHANGES IN OLD AGE.

Just as it may be difficult to state dogmatically that an old person is purely in a state of physiological senescence as opposed to a condition in which past illnesses and infections have played a part so that there is a combination of physiological and morbid influences —of senescence and senility—so it may be hard to draw a hard-and-fast line between the structural changes of physiological involution and atrophy and those that may be regarded as pathological, especially as disease may accelerate the involutionary changes. It is probable that the ideal of physiological involution is so rare that at best the changes are but relatively physiological. The general atrophy is shown by loss of weight, for how rare it is that a really old person is fat, in fact obesity is a sign that the normal metabolism, or exchanges in the body, is not physiological. The atrophic process does not proceed equally in all the constituents of the body; the nobler active cells are much more affected than the supporting fibrous tissues. The nerve cells in the brain and spinal cord become small, degenerated, and pigmented, and the brain as a whole weighs less. Arcus senilis, though occasionally seen in early life, is like grey or white hair an accompaniment of age and due to degeneration; changes in the crystalline lens cause presbyopia, which may be brought on prematurely by toxæmia, it is said, of intestinal origin (E. Clarke). The fall of the hair is not necessarily confined to the aged; a rare, but curious, effect is that after illness naturally brown hair has been reported in place of that previously white. The skin becomes dry, glossy, inelastic like parchment, and wrinkled from atrophy of the fat, muscle, and elastic tissue, especially on the backs of the hands. From diminution of the blood supply there is an ivory pallor somewhat relieved by pigmented areas and dark brown patches of seborrhœic eczema; when in such a condition of impaired resistance the skin readily becomes damaged and is prone to infection, so that pruritus and even indolent ulcers may result. From atrophy of fat the veins become more prominent, and the appearance of small red angiomas on the trunk is common in middle age.

Like the hair the teeth become few; Sir G. Humphry found that above eighty years of age the average number left was six in men and three in women; it may not be so many now. Sir Isaac Newton, however, at the age of eighty-five was said to have lost only one tooth. The loss of teeth might perhaps be regarded as a hint that there is no longer need for so much food. With the loss of teeth the lower jaw returns to its shape in the infant, hence the "nutcracker" aspect of the edentulous face. The bones of the skeleton get thinner, but retain their length and usually their form. The back becomes bowed from muscular weakness, but the cartilages of the ribs and the larynx do not become calcified except as a pathological event comparable to calcification of the arteries. It is interesting to speculate if there is any relation between the atrophy of bone and the enlargement of the prostate which occurs in a certain number of elderly men; for Grove and Vines found a deficiency of calcium in the blood of patients with enlarged prostates, and obtained benefit by giving extract of parathyroid gland, which controls calcium metabolism.

The heart is probably less altered than the other organs and tissues of the body, and indeed its efficiency must be essential for prolonged existence, as may be shown by statistics to the effect that from sixty to ninety years of age death is more often due to failure of the circulatory system, sometimes a blessed painless passing away in sleep, than to any other cause, whereas before sixty failure of the respiratory system is most responsible; to keep the heart in a healthy condition exercise is important.

The blood of healthy octogenarians does not necessarily show any change in the number of the red blood corpuscles and the amount of hæmoglobin, though pallor of the skin may suggest an anæmia which does not exist. Sometimes there is secondary anæmia resembling the Addisonian type; this is either due to morbid influences or to a more advanced atrophy and involution of the red bone marrow than usually occurs. The lymphatic glands, the spleen, and the leucoblastic bone marrow all, like the other organs, diminish in size, but this does not appear to influence the leucocyte count.

As already mentioned arterio-sclerosis is neither the necessary accompaniment nor the cause of healthy old age. But it is very frequent in old people, and often causes premature senility, renal disease, and difficulty in walking (intermittent claudication or limp).

The endocrine glands share in the general involution, but in spite of attempts to find in them, and especially in the thyroid and the interstitial cells of the gonads, the elixir of life, this desideratum is still to seek.

The sexual organs slowly atrophy, with the exception that the prostate gets larger in most men after the age of fifty, though a small percentage only of them suffer from it. The formation of fibromyomatous tumours of the uterus and of cystic involutionary changes in the mammary glands of women appears to be analogous to that in the prostate.

HEALTHY AND PATHOLOGICAL OLD AGE.

From the time of Terence, Cicero, and Sanatorius, old age has been regarded as a disease, and Samuel Johnson remarked, "My diseases are an asthma, a dropsy, and, what is less curable, seventy-five." But it is important to remember that there are two kinds of old age: (i) The healthy old age, which I almost called the normal, but perhaps, as this might imply that it is the usual or average form, it is better not so described; and (ii) the commoner, in which the body has not simply grown old, but shows the relics and results of past disease, accumulated in the passage of years. It has been said, and with fair probability, that most people over sixty years of age have some focus of infection, such as those in connection with the teeth, the accessory nasal sinuses, the prostate, or gall bladder; these sources of poisoning undermine the general health and age the tissues, especially when they are beginning to undergo the natural process of involution and atrophy. To draw a hard-and-fast line between healthy old age (senescence) and old age partially caused by some disease may, in individual instances, be difficult or even impossible; they so often overlap. It is, however, observance of pathological old age that accounts for the pity, if not dislike, with which it is commonly viewed, and for the opinion expressed by many people that they have no wish to live to a great age.

The influence of adverse conditions, unhealthy surroundings, infections, and disease in shortening life and producing a pathological, that is unnatural, old age is seen by comparing the physiological existence of man, which not unreasonably may be put at a hundred years, with the average expectation of life at birth in England and Wales—55·35 years for females and 51·5 years for males. The age of fifty to sixty is that when some common diseases, such as arterio-sclerosis, failing heart, kidney disease, cerebral hæmorrhage, liver disease, and cancer, take a heavy toll. Those who survive with cardio-vascular disease are prematurely restricted in their activities and so help to increase the unhappy impression that advancing years has in the popular mind.

There are many definitions of disease, or want of ease, and as good a one as any is a want of adjustment between the individual and his surroundings. The frequency with which those advanced in years suffer pain and discomfort is evidence of more than length of days; for example, rheumatism is the result of some focus of infection. Arterio-sclerosis, the result of past or present high blood pressure or of poisons—bacterial, metabolic, and of intestinal origin—is very common in old people, but is not, at least in an extreme degree, a necessary accompaniment.

THE DESCRIPTION OF OLD AGE IN THE TWELFTH CHAPTER OF ECCLESIASTES.

In connection with old age every one must recall the famous description in the first six verses of the twelfth chapter of Ecclesiastes, beginning "Remember now thy Creator in the days of thy youth while the evil days come not, nor the years draw nigh when thou shalt say I have no pleasure in them." Formerly ascribed to King Solomon (977 B.C.) the book of Ecclesiastes (in Hebrew Koheleth = the preacher) has been shown by the higher criticism to date only from the end of the third century B.C. In his attractive work, *A Gentle Cynic*,* the late Professor Morris Jastrow, jun., of Philadelphia, argued that the book of Ecclesiastes as it appears in the authorized version, consists of (i) the original, cynical, but good-natured *obiter dicta* of the unknown dilettante who preferred to veil his identity under the name of Koheleth; and (ii) additions and modifications made by various hands to render it more orthodox and compatible with the tradition that it was written by Solomon. Thus the admonition "of making books there is no end, and much study is a weariness of the flesh" may very probably have been intended as a hint that Koheleth's views should not be taken too seriously. Following this conception Jastrow reconstructed the text of the book of Ecclesiastes to what he argued was its original form, and compared it with the more modern writings of Omar Khayyám and Heinrich Heine. As we all must have speculated over the correct interpretation of the various metaphors in this description of the last stage of life, the explanations offered by others, such as Andreas Laurentius* (1599), Master Peter Lowe† (1612), founder of the Faculty of Physicians and Surgeons of Glasgow, Bishop J. Hall ‡ (1633), Richard Mead § (1775), and Jastrow may be very briefly mentioned. In 1666 John Smith devoted a book of 266 pages to elucidate these six verses which contain 207 words—*King Solomon's Portraiture of Old Age wherein is contained A Sacred Anatomy both of Soul and Body.* He is peculiar in authorship on this subject at his early age, thirty-five years, for nearly all the other writers on this topic, such as Cornaro, Sir Anthony Carlisle, Charcot, Sir George Humphry, Sir Hermann Weber, and Robert Saundby, not to mention and embarrass those happily with us, have been approaching "the sere, the yellow leaf," and have perhaps been moved to their labours by the maxim, "Physician, cure thyself."

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* *The Gentle Cynic, being a Translation of the Book of Koheleth, commonly known as Ecclesiastes, stripped of later Additions, also its Origin, Growth and Interpretation*, by Morris Jastrow, junior, 1919, J. B. Lippincott Co.

The second verse, "While the sun, or the light, or the moon, or the stars, be not darkened, nor the clouds return after the rain," is regarded by Laurentius, Lowe and Hall as referring to the ocular disabilities of old age, whereas Smith and Mead consider that mental failure and depression are meant. As regards the third verse, "In the day when the keepers of the house (the hands) shall tremble, and the strong men (the legs) shall bow themselves (become bent), and the grinders (teeth) cease because they are few, and those that look out of the windows (the eyes) be darkened," there is general agreement, Lowe specially designating cataract as meant in the last sentence. "And the doors shall be shut in the streets" is regarded as referring to the mouth by Laurentius and Mead, and to the various orifices, including the results—constipation and dysuria—by Smith; "when the sound of grinding is low" is considered by Jastrow to mean impaired hearing, and by Smith as a lowered rate of metabolic processes, such as assimilation, blood formation, and various secretions. "And he shall rise up at the voice of the bird" implies, according to Smith and Mead, the early waking of the elderly; "and all the daughters of music shall be brought low" signifies to Laurentius the failure of voice, to Mead deafness, and to Smith all the organs concerned with sounds—namely, the lips, tongue, larynx, and the auditory apparatus. "Also when they shall be afraid of that which is high, and fears shall be in the way" is regarded by Smith as describing the general mental attitude of anxiety for things both small and great and a bad head for height; but a more modern commentator suggests that "afraid of that which is high" refers to dyspnœa on climbing a hill. "And the almond tree shall flourish" is by Laurentius, Hall and Smith thought to refer to the white hair or "churchyard flowers" of the old, but Mead argued that loss of smell is meant. "And the grasshopper shall be a burden" has been very variously interpreted: Hall is content to accept the literal meaning that the least weight is a nuisance; Laurentius and Lowe understand œdema of the legs; John Smith that the aged body undergoes the reverse change of shrivelling, hardening and angularity. In the sixth verse the words "Or ever the silver cord be loosed" refer, according to Laurentius, Lowe, Mead and Jastrow, to kyphosis, but Smith translates them into paralysis of the spinal cord and nerves. "Or the golden bowl be broken" signifies cardiac failure to Laurentius and Lowe, but cerebral hæmorrhage to Smith, who thus explains the next line, "or the pitcher (the veins) be broken at the fountain (the right ventricle), or the wheel (the arterial circulation) broken at the cistern" (the left ventricle), and therefore concludes that King Solomon was perfectly acquainted with the circulation of the blood discovered by William Harvey in 1616.

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* Laurentius, A., *A Discourse of the Preservation of the Sight; of the Melancholic Diseases; of the Rheums; and of Old Age*, translated out of French into English by Richard Surphlet, 1599.

† Lowe, P., *The Whole Course of Chirurgerie*, 1612.

‡ Hall, J., *Paraphrase upon the Hard Texts of the Whole Divine Scripture*, 1633.

§ Mead, R., *Medica Sacra*, translated from the Latin by Thomas Stark, 1755.

The diseases of old age would make a long chapter. In each of the ages of man's strange eventful history some diseases are particularly prominent, and others very unusual or never occur; but generally speaking the influence of age consists in modifying the character rather than in initiating special forms of disease. A convenient method of considering the subject would be under the two heads of diseases (i) occurring in the aged, and (ii) those more or less confined to the last phase. Diseases common to all or most stages of life are in old age less acute, less dramatic, and more prone to be latent: in Charcot's words the organs suffer in silence without any local or general disturbance, though sudden collapse may occur. Thus pneumonia, which Osler called "the friend of the aged," because by a short and rarely painful illness it relieves them from those "cold gradations of decay" that make the last stage of all so distressing, is often so latent that death is ascribed to senile decay. There is, as already pointed out, a great contrast between early and late life in the reaction to disease ; but in acute disease, contrary to what might be expected from what has been said about the sluggish response, there is no tendency to absence of fever in the aged. In life's journey the scars and effects of disease accumulate so that rheumatoid arthritis and crippling arterio-sclerosis and its results, and some forms of cancer, become more prominent. Dr. T. H. C. Stevenson, of the General Register Office, has kindly provided me with statistics showing that cancer is not a disease of the period from fifty to seventy, rather than of the more advanced old age ; in fact recent returns, with a few apparent and doubtful exceptions, show that on the whole mortality from cancer of the more accessible sites continues to increase up to extreme old age, whereas that from the less easily accessible does not, the decrease in extreme old age being greatest for cancer in parts of the body—the stomach and intestines —where it is more likely to escape detection.* In America it has been estimated that 10 per cent. of all old people die from unsuspected cancer (Wood).† It has, however, been argued that old age alone does not specially favour the development of cancer, and that the relationship is entirely indirect by providing opportunity for long-continued irritation (Murray).‡ In old age repeated exposure to the infections responsible for fevers, such as measles, scarlet, diphtheria, and typhoid, has produced a considerable degree of protection. Pneumonia and erysipelas, however, are prone to recur.

Many of the affections of the old are the result of arterio-sclerosis, a legacy of the middle period of life, which has continued to progress, and by interfering with the circulation produces serious degeneration in the important cells of the organs, especially in the central nervous system : thus from changes in the brain and spinal cord there result senile dementia and various forms of paralysis, and from implication of the arteries of the lower limbs difficulty in walking from cramp and intermittent claudication, and senile gangrene. There is a form of tremor in the elderly which often affects the head, and paralysis agitans or Parkinson's disease—so called after its describer James Parkinson, a humble general practitioner in Hoxton, who was also a palæontologist and author of pamphlets advocating bloodless revolution—though not confined to old age, is most usual then ; it is of particular interest because its features are closely imitated by the result of encephalitis epidemica, which since the War has been comparatively common in the young, and so is a good example of the degenerative effects of disease imitating those seen in advanced age.

[H. R.]

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* Stevenson, T. H. C., "The Registrar-General's Statistical Review for England and Wales, 1923," text, p. 69.

† Wood, F. C., *Journ. A.-M. Med. Assoc.*, Chicago, 1919, lxxxiii. 569.

‡ Murray, J. A., Eighth Scientif. Rep. Imperial Cancer Res. Fund, p. 79, 1923.

WEEKLY EVENING MEETING,

Friday, May 20, 1927.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. M.V.O. C.B.E.,
President, in the Chair.

W. L. BRAGG, O.B.E. M.A. F.R.S., Langworthy Professor of
Physics, Victoria University of Manchester.

The Structure of Silicates.

AT a time when the fundamental conceptions of the structure of matter are being changed so rapidly, when every six months witnesses the birth of a new analytical method of dealing with the very foundations of our physical science, the study of the particular crystal patterns which form the subject of my lecture must seem a trivial matter. Yet similar studies have played their part in the extraordinary development of physics in the last decade, and I hope that the refinement of our methods of analysis, such as is represented by the present examples, will in turn prove to be of use. The results of the particular investigations which I propose to describe are interesting in themselves because the silicates form so large a proportion of the earth's solid crust, and certain artificial forms are so largely used for technical purposes. I think, however, that it is right to stress another aspect of this study. We are trying to improve the technique of the X-ray examination of solid bodies, to increase the resolving power of our instruments so that we can see finer detail and deal with more complicated structures. It is at present a tedious and difficult task to discover how the atoms are arranged in these bodies; even when some experience in handling them has been obtained one has to devote much time and concentration to each particular case. Yet every solution makes the next problem easier to attack, and when we look back on the last few years' progress I think a very real increase in power of analysis is evident as the result of the efforts of many workers in this field. It is this advancement of technique in which personally I am particularly interested. In trying to improve our instrument we examine with it from time to time a new type of solid body—these silicates being an example—and we describe what we see. I am not competent to discuss the discoveries we make; I can only hope that the casting of light from a new angle may be useful to those who have made a life-long study of these particular types of compound. I feel that my main contribution must be a demonstration of what knowledge it is possible to attain by careful X-ray examination.

2. The silicates present a highly interesting series of problems for X-ray analysis. The numerous crystalline forms have been carefully studied because of their importance to the mineralogist, and they show most interesting relationships and wide variation in composition. It is estimated that oxygen, silicon, aluminium and iron by themselves compose about 87 per cent. of the earth's crust, and if we add four other elements, calcium, sodium, potassium and magnesium, they amount together to 98 per cent. These are the elements that build up the compounds we are considering, and their relative proportions are a reflection of the fact that most of the earth's crust is composed of compounds of metals with silicon and oxygen.

The silicates occur as solid bodies, whose atoms are arranged in crystalline patterns. These patterns are often very complex, and it would be difficult to attack them by general methods unless some guiding line could be followed through the intricate maze. Such a guiding line may perhaps be found in the peculiar part which the oxygen atoms play in building up the structure. Not only are the oxygen atoms the most numerous, but they appear to be the most bulky of the units of which the pattern is woven, so that their predominant size and number make them force the other atoms to conform to certain simple and characteristic arrangements of oxygen atoms which we find occurring again and again as an underlying *motif*, throughout the range of such silicates as have yet been analysed. A few simple examples may help to illustrate this point.

Four spheres, packed together as tightly as possible, assume a tetrahedral arrangement with one sphere standing on three others (Fig. 1*a*). Six spheres when packed together take up a form where three of them lie on top of three others (Fig. 1*b*). Alternatively we may regard this arrangement as four spheres at corners of a square, with one above and one below this square (Fig. 1*c*), the second arrangement being identical with the first regarded from a different view point.

These very simple groupings of oxygen atoms, with an atom of some other element at the centre of the group, occur again and again in the silicates and in many other compounds. In many cases the group not only has a characteristic shape, but also a characteristic size, the distance between the oxygen atoms having a value of about 2·5 to 2.7 Ångström units (10^{-8} cm.). In the list of common elements given above it is only sodium, calcium, and potassium which appear to break up the regularity of this group, and to force the oxygen atoms apart if placed at the group centre. The other elements appear to fit comfortably into the interstices of the oxygen grouping. Since there is a common distance throughout between oxygen atoms, certain atoms of one group can at the same time form part of the next group, and so a continuous structure is built up which may be thought of as a pattern of oxygen atoms with the metal and silicon atoms in its interstices.

3. This predominance of the oxygen atoms greatly simplifies the analysis of the structures, and makes it easier to visualise the relationships between different types. All these bodies build up a crystalline pattern repeated again and again in space. Now the simplest types of pattern are those such as children are taught to make in the kindergarten, where blocks of the same shape and size, but coloured differently, can be stacked together in geometrical designs. The silicates present rather a fascinating analogy to these kindergarten patterns, the oxygen atoms being the blocks and the other atoms the colouring agent. It is as if a complex pattern were embroidered by the other atoms upon a simple underlying fabric of oxygen atoms.

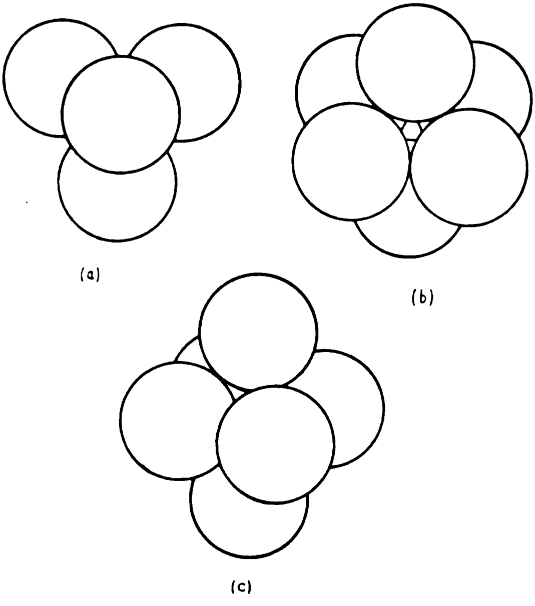


Fig. 1.—Groups of four, and of six, oxygen atoms. Such groups, with atoms of silicon or one of the metals at the centre, are repeated indefinitely in the silicate structure.

The two types of group, four-fold and six-fold, illustrated in Fig. 1 are composed of oxygen atoms equidistant from their neighbours. We have seen that they may be regarded as spheres compressed into the smallest possible space. Such an arrangement can be continued indefinitely, and if it is done regularly one or other of two characteristic groupings of spheres is the result. These are the well-known forms of cubic and hexagonal close-packing.

This close-packing of the oxygen atoms is a very simple arrangement, and it is interesting to find that a number of silicates are based on it. Its existence in a silicate can be surmised by noting how much volume there is to each oxygen atom in the whole structure. If the oxygen atoms are packed together closely, with 2·7 Å. between their centres, it is easy to calculate that each atom occupies a minimum volume of 14 Å^3. (If magnesium and iron are present, they expand this volume slightly by an amount for which allowance can be made.) Further, the refractive index, if ideal close-packing exists, must not be less than 1·7, as the oxygen atoms have a high refractivity.

Using these tests, it appears probable that certain compounds are based on one of the forms of close-packing, and we have examined some of these cases. The close-packing of the oxygen atoms is, however, exceptional, and although in most compounds the four-fold and six-fold groups form the basis of the pattern, this pattern may be of an open type. The various forms of silica, to whose investigation the Royal Institution has made so large a contribution, are beautiful examples of open patterns built of the four-fold groups, and other examples of these lace-work open patterns have been analysed. The closely woven patterns have been chosen for description here because their basis is the more simple.

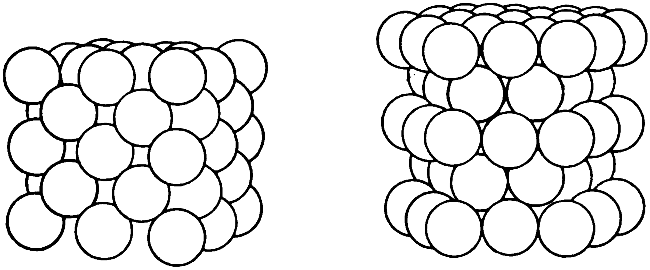


FIG. 2.—Spheres in cubic and hexagonal closest-packing.

4. In order to understand these frameworks of oxygen atoms, with metal atoms packed in the interstices, it is necessary to bear in mind some of the geometrical laws of pattern-making. If a repeated dattern be formed by stacking together blocks of the same size and shape, but different in colour, the unit of the pattern will be composed of a whole number of blocks. It is always possible to outline the pattern with a series of unit cells, each of which just contains one complete example of the pattern and no more. The unit cell may be a large and distorted one if the pattern is complex, but it

must always obey one condition. If one corner is placed at the centre of a block, all the other corners will also be at centres. The crystals which are based on one of the close-packed arrangements of oxygen atoms must have a unit cell related to the close-packed arrangement in this way.

Disthene or cyanite, Al_2SiO_5, is an example of such a crystal. Its unit cell could not well be more irregular. Its edges, and the angles between them, are all unequal, as shown in Fig. 3.

Yet its high refractive index (1·72) and small volume per oxygen atom (15·0 Å^3) hint that the oxygen atoms are in one of the forms of close-packing. An X-ray examination has proved this to be the case. The packing turns out to be of the cubic type, and the way the disthene cell and the cubically arranged oxygen atoms "fit together" is shown in Fig. 3. The cell of disthene contains twenty oxygen atoms, and the cell outlined in the right-hand figure contains exactly twenty close-packed spheres. This very complex pattern of disthene has to be woven into a basis of twenty oxygen atoms, since a multiple of five is demanded by the chemical formula. The irregular disthene cell is the way chosen by Nature of blocking out suitable groups of twenty oxygen atoms from the very simple cubic structure.

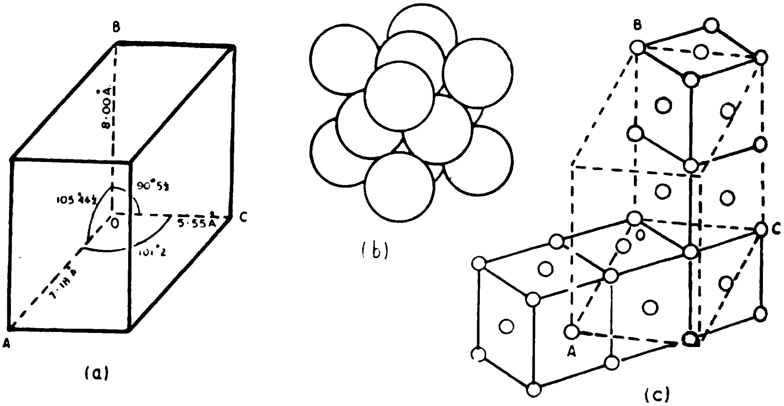


FIG. 3*a* shows the unit cell of disthene. FIG. 3*b* is a portion of the array of spheres in cubic close-packing. FIG. 3*c* shows to the same scale a more extended portion of this array, the centres of the spheres alone being indicated. The unit cell of disthene is also a unit cell (containing twenty points) of the cubic lattice; this is proved by the way in which OA, OB, OC correspond in length and direction in Figs. 3*a* and 3*c*.

When the scattering of X-rays by the crystal is examined, the close-packed arrangement of oxygen atoms shows up strongly. We can consider the effect on the X-rays as composed of that due to the oxygen atoms on the one hand, combined with that due to the atoms of metal and silicon on the other hand. The former leads to a simple and intense diffraction pattern, to be expected from a straightforward cubic face-centred lattice. The aluminium and silicon atoms, which form a complex embroidery on a large scale woven into the oxygens, give a complex diffraction pattern within that due to the oxygen atoms alone. Fig. 4 illustrates the point.

The complex inner pattern contains the information necessary to tell us where the aluminium and silicon atoms are. It is a difficult matter to unravel its story, but it is by no means as formidable as it would have been, had we not known that the oxygen atoms are nearly in this simple arrangement, and that the aluminium and silicon atoms are somewhere within the four-fold or six-fold groups of oxygen atoms.

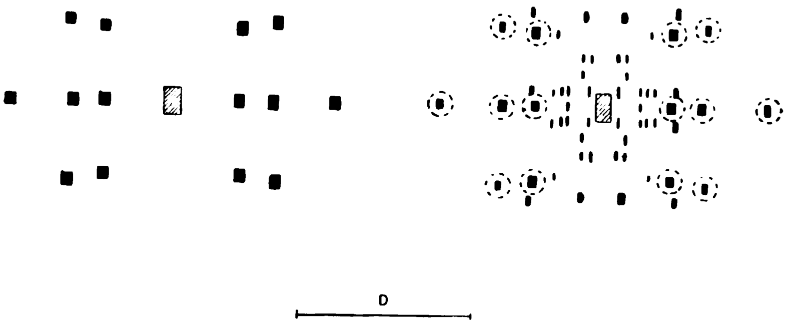
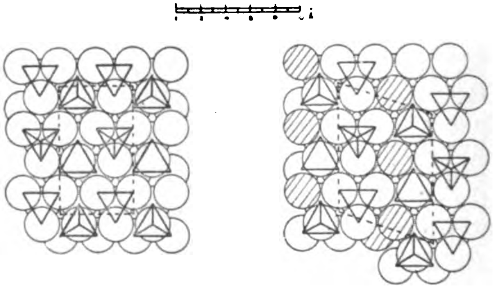


Fig. 4.—Rotation photograph around *b* axis of disthene (right) compared with ideal rotation photograph around cube edge for close-packed atoms (left).

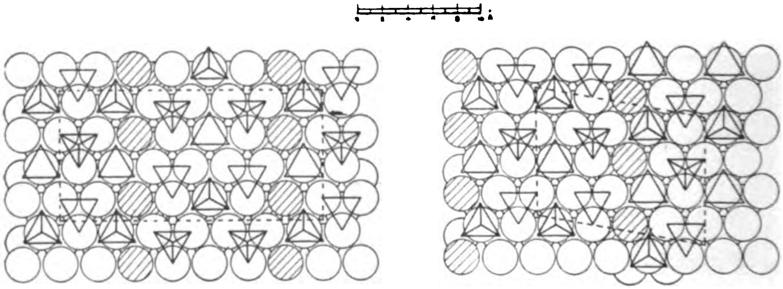
5. As another example of this pattern-weaving, the series of compounds Mg_2SiO_4, $MgO_2H_2\,(Mg_2SiO_4)_2$, $MgO_2H_2\,(Mg_2SiO_4)_3$, $MgO_2H_2\,(Mg_2SiO_4)_4$ may be examined. The dimensional relationships between these crystals (the chondrodite series) have long aroused interest. I have to acknowledge very gratefully the loan by Sir William Pope of a model to illustrate these relationships which he showed at the Royal Institution seventeen years ago. If we measure the unit cells of the compounds we find that two axes, outlining one face, remain constant throughout all four crystals. The thickness of the cell measured perpendicularly to the face increases in regular steps in the last three compounds, as if blocks of magnesium silicate were being added on in a regular way. With the aid of X-rays this process can be followed out in detail, and some finer points of it are not without interest. All the compounds

prove to be based on hexagonal close-packing, and with the aid of this guiding feature Mr. West and Mr. Taylor, who have been working on these crystals in my laboratory, have, in my opinion, succeeded in elucidating the approximate positions of all the atoms in these complex patterns. Their results are shown in Fig. 5.



a Olivine

b Chondrodite



c Humite

d Clinohumite

Fig. 5.—Atomic arangement of the chondrodite series of minerals (after Taylor and West). Two layers of oxygen atoms in hexagonal close-packing are shown, projected on the plane (100) of each crystal. The "*a*" axes of the crystals are perpendicular to the plane of the diagram, and are practically identical for all the crystals. The diagram shows the identity of the vertical "*b*" axis in all the crystals, and the relationships in direction and length between the "*c*" axes.

It is impossible not to feel apologetic when asking an audience to follow the intricacies of these patterns, which are very formidable unless one has made a special study of them. However, the figures may perhaps make clear the main points. We have to explain the way in which the unit cell varies in size from compound to compound, and we have also to explain a curious complication. Whereas Mg_2SiO_4 and $MgO_2H_2\,(Mg_2SiO_4)_3$ have a rectangular cell (orthorhombic), the other two compounds are built on a slant with the type of symmetry called monoclinic. The blocks are not clapped directly on top of each other, but are stepped sideways. It is fascinating (to the enthusiast) to see how naturally the results follow from the arrangements shown in the figure.

The circles in the figure represent the oxygen atoms which form the framework on which our whole structure is based. Two layers of such atoms are shown, representing two sheets of spheres packed closely, one lying on the other. Some of the oxygen atoms are linked together to form a group SiO_4 with a silicon (not shown) at its centre. These groups are shown as tetrahedra in the diagram, and six or three edges of each tetrahedron are drawn, depending on whether its apex is turned towards or away from the observer. The magnesium atoms are left out of the diagram for the sake of simplicity; the shaded pairs of oxygen atoms are to be labelled "OH," since they must belong to the hydroxyl groups.

Each crystal is a series of alternate strata of magnesium silicate and magnesium hydroxide. That part of it which is magnesium silicate is arranged exactly like the pure magnesium silicate shown in Fig. 5*a*. The layers of hydroxide cement together the blocks of magnesium silicate. In order to outline the unit cell of any of these patterns, we join up four points in the pattern which are exactly alike. The diagram will show that the measured unit cells, shown by dotted lines, are exactly those cells which satisfy these conditions. Chondrodite, $MgO_2H_2\,(Mg_2SiO_4)_2$, must have a slanting cell, the next compound humite a long rectangular one, and the last, clinohumite, again a slanting cell. We were surprised in making our X-ray examination to find such a curious difference in the shape of the unit cell between humite on the one hand and chondrodite and clinohumite on the other hand. When the pattern was put together, however, it was clear that such a difference followed naturally from the relative numbers of silicate and hydroxide strata.

6. Though very few examples of silicates have yet been analysed, the temptation to draw conclusions from them is irresistible. The most curious feature about the natural silicates is the immense variety in composition which occurs within one and the same mineral species. Stupendous chemical formulæ have to be assigned in order to explain even approximately the relative proportions of the different elements in some well-known types.

It seems to me that the position becomes clearer when we consider the compounds as an embroidery of the metal atoms upon an oxygen framework. We may compare the oxygen framework to the steel girder system of a large ferro-concrete building in course of construction, which is intended to be divided into sets of flats.

Before the girder system is filled in, its configuration is very simple indeed. Then certain blocks of it are partitioned off into sets of rooms, each comprising a flat; these flats are the more complex units of pattern in the crystalline structure. The arrangement of the rooms in each flat corresponds to the selection in our oxygen framework of certain spaces in which to put the metal atoms. We can go one step further, and rent our flats to families of slightly different composition, so that a census of the whole building shows a bewildering proportion of types of inhabitants difficult to represent by a definite family formula. This is really the state of affairs with which the chemist is confronted when he attempts to give a formula to many of the silicates.

Some of the very earliest structures which were analysed caused us to revise our ideas of what was meant by the "molecule" of the chemist. In sodium chloride there appear to be no molecules of NaCl. The equality in numbers of sodium and chlorine atoms is arrived at by a chess-board pattern of these atoms; it is a result of geometry and not of a pairing-off of the atoms. This is, of course, not universally true, for this absence of the molecule in solids is in general only found in inorganic compounds. It would appear, however, that the silicates are of this non-molecular type, and that in seeking to assign formulæ to them, and to the hypothetical acids of silicon on which they are based, it should be borne in mind that they are really extended patterns. The relative numbers of their constituent atoms are characteristic of the extended pattern, and essentially a result of their solid state, so that it is doubtful whether a grouping of the atoms into molecules has in this case a meaning. It will be very interesting to see what further light the X-ray results can cast on the relationships in this fascinating series of compounds.

[W. L. B.]

WEEKLY EVENING MEETING,

Friday, May 27, 1927.

His Grace The Duke of Northumberland, K.G. C.B.E. M.V.O.,
President, in the Chair.

His Excellency The Marques de Merry del Val,
G.C.V.O. LL.D.,
Spanish Ambassador at the Court of St. James's.

José Maria Gabriel y Galan: A Contemporary Spanish Poet.

[Summary.]

1. *Introduction.* A brief review of Spanish Literature.
    - (*a*) Antiquity of Spanish Literature.
    - (*b*) Extent of Spanish Literature.
    - (*c*) Consciousness of early Spanish Literature.
    - (*d*) Deliberate reform of early Spanish Literature.
    - (*e*) Points of resemblance between the Literatures of Spain and England.
    - (*f*) Modern Spanish Literature, with special reference to Gabriel y Galan.

2. *Discourse on Gabriel y Galan.*
    - (*a*) Intellectual figure of Gabriel y Galan.
    - (*b*) Life of José Gabriel y Galan.
    - (*c*) Virgilian quality of Gabriel y Galan. His resemblance to Robert Burns.
    - (*d*) Atmosphere surrounding Gabriel y Galan.
    - (*e*) Brief analysis of Gabriel y Galan's poems: their bucolic, lyrical and religious character.
    - (*f*) A few examples of Gabriel y Galan's work.

[de M. del V.]

Note.—The subject matter of the Discourse appears in the Bulletin of Spanish Studies, Vol. IV., No. 13, January 1927, p. 3 (University of Liverpool), being the report of a Lecture delivered at Glasgow before the Spanish Society of Scotland.

WEEKLY EVENING MEETING,

Friday, June 3, 1927.

J. Mitchell Bruce, C.V.O. M.D. LL.D. F.R.C.S.,
Vice-President, in the Chair.

George M. Trevelyan, C.B.E. LL.D. F.B.A.

Carlyle as an Historian.

As I sat down one morning in Chelsea to compose this address on Carlyle as an historian, the genius of the place and subject gar'd me groan and lament my fate, shut up with a pen and blank paper, at such an unconscionable task. *Ay de mi!* And yet courage! Lesser men with good digestions have no right to pretend that literary composition is not, upon the whole, one of the pleasantest forms of work in this world. My special reason for groaning over the subject, which I admit I chose for myself, was not that it is antipathetic to me—far otherwise!—but rather that it is all too sympathetic. For five and thirty years, as I reckon, Carlyle's historical writings have given me an amount of intellectual happiness which I cannot accurately compute, and which no one else can even guess at who has not had the same personal experience. In the inner study of my own mind, where not even one's own most intimate friends can ever wholly penetrate, I have held converse with Carlyle as an historian, "frae mornin' sun till dine" of this my life's day. And now, in a rash moment, I have promised your secretary to speak on the matter, "to read a paper" on it, ye gods!

How am I to do that without doing injustice to Carlyle, and not leave a botched piece of work indeed as compared to the real truth of it all?

But do not mistake me. Lover as I am of Carlyle, I am very well able to be his critic also. I am old enough to have outgrown the habit of loving an author's very faults, as generous youth does and should do in the case of its idols, provided its idols are, upon the whole, well chosen. As an historian Carlyle has immense faults both of omission and commission. But his faults are just those which modern historians are in least danger of imitating, and his merits are precisely those in which modern history is so lamentably deficient.

Now in the first place Carlyle held, rightly or wrongly, a much higher estimate of History than other historians, or other men of letters. To his thinking, fiction was an inferior, that is a less valuable, part of letters than History, because, as he believed, Truth about actual facts was not only more instructive but more poetical than fiction. I shall return to that thesis of his later, and exemplify his meaning and discuss its validity. Enough for the moment that he thought so. To him "men's History and man's History" are "a perpetual evangel," and the events of the Past, if truly recorded, are the message of God to man. Perhaps after all he was right; perhaps that is so. Yet it is prudent to admit that God's handwriting in History is often most illegible, and that His message is more manifold and more complex than even Carlyle saw, than any one of us is able to see. Each of us sees a glimpse, deciphers a sentence or two in that great Book of Life, or Bible, which History is, or would be if it ever came to be rightly written. Yet I wish that every modern student would begin with something of the spirit of Carlyle's awestruck approach to History, his deep reverence for the record of the Past of the human race, as being the true poetry, the true religion.

Cause and effect! Of course History is a record of cause and effect, of evolution, of the quasi-miraculous *growth* of civilisation out of barbarism, of each new form of life and thought emerging out of that which went before—the moving picture of mankind ever on the march from the unknown origin to the unknown fate:—"on the hardest adamant some footprint of us is stamped in: the last Rear of the host will read traces of the earliest Van,"—on barrows of the downs and moors, and in gravel of cave-floor and river-drift.

But History is more even than a record of evolution. It is, or should be, also the record of the heroic things that men have thought and done in their hard, often baffled, never abandoned struggle upward out of darkness to light. It is the History of the best men and of the best in ordinary men, dreadfully set off against the background of the worst and the most commonplace which so often swallows up the best. but never wholly swallows it.

Why, then, if History is all this, does it hold a place so much inferior not only to poetry but even to novels in the estimation not only of the multitude but also of the more knowledgeable literary world? There is at least one answer that is in itself sufficient. The men of genius have preferred to write poetry, novels and *belles lettres* instead of History. Carlyle and one or two more are the sole exceptions; let us be glad of them instead of trying to drum them out of the historical army by proclamation, as was the fashion among professional historians a generation ago. We shan't get too many men of the calibre of Carlyle to write History, so there is no need to discourage them. The notice board "No Man of Literary Genius need apply" was equally foolish and unnecessary. It has now been taken down, but the men of literary genius have not rushed in.

But if Dry-as-Dust was at one time hard on Carlyle, it must be admitted that he had provocation. Carlyle while he used the work of Dry-as-Dust, groaned aloud over him and abused him all the while, which was hardly fair, and naturally set up bad feeling. Read, for instance, what Carlyle writes at the end of the first chapter of *Frederic* :—

> "It is frightful to see the Gelehrte Dummkopf (what we here may translate *Dry-as-Dust*) doing the function of History, and the Shakespeare and the Goethe neglecting it. Interpreting events, interpreting the universally visible, entirely *in*dubitable Revelation of the Author of this Universe: how can Dry-as-Dust interpret such things, the dark chaotic dullard, who knows the meaning of nothing cosmic or noble, nor ever will know? . . . . They that could have taught better were engaged in fiddling; for which there are good wages going."

This passage, to my mind, contains a core of all-important truth in a setting of error, ill-temper and injustice. Dry-as-Dust has often a dumb feeling for the poetry and nobility of the facts of the past, which leads him to devote his life to historical learning, instead of going elsewhere for the "good wages" of "fiddling" or other more highly rewarded pursuits. Dry-as-Dust, if he had no emotional feeling for his subject, deep and silent in his heart, would not devote his life to it. He loves to edit the "chronicles of wasted time," in order to catch a glimpse of the real life of "ladies dead and lovely knights," even if it were only by discovering the nature of the lovely knight's fees.

The passage which I have quoted from Carlyle is, perhaps, equally hard on the men of genius, whom it accuses of having chosen to "fiddle" instead of to write history. The fact is that Shakespeare and Scott each did far more for history by writing historical plays and romances than if Shakespeare had written the Chronicle of the Kings of England, and Scott (heaven save us!) had written a series of histories like his *Life of Napoleon*. Both Shakespeare the play-writer and Scott the novelist interpreted God's ways with man as revealed in history no less effectively than Carlyle himself. The popular interest in history to this day owes more to Shakespeare and to Scott than to any two other men. In our own times Thomas Hardy with his *Dynasts* and Bernard Shaw with his *St. Joan* have also helped history in the best way each could. A history of the world by Mr. Shaw might be as wayward and ephemeral as one of his own Prefaces; but *St. Joan* is a work of art and an important piece of historical interpretation. Hardy's *Dynasts* stands in a class with Shakespeare's historical plays, and though it lacks Shakespeare's unique genius, it is nearer to the actual truth of history, because Hardy lives in a more historically-minded age.

And yet we may be very thankful that among the men of literary genius Carlyle for one did devote himself to the writing of History in the strict sense of the word. For his history books have more permanent value than his pamphlets and his philosophical and political lucubrations—*Sartor* excepted. Take for instance *Past and Present.* In that little volume how much more valuable, at least for our purposes to-day, is his treatment of the Past than his treatment of the Present. The story of St. Edmonsbury Monastery loses singularly little in the light of the greater knowledge we have to-day acquired of the Middle Ages. But the rest of the book, a pamphlet on the social distress of the hungry 'forties, about Sir Jabesh Windbag and the Captains of Industry and so forth, was written for his age, not for ours. His Abbot Samson, on the other hand, is for all time. Or again, compare the permanent value of his *Cromwell* to that of his ephemeral *Latter Day Pamphlets.* Carlyle found his true métier as an historian.

But before I begin to discuss his historical works *seriatim*, I should like first to go back to his theory of history, and examine more fully his reasons for preferring of History over fiction and even over poetry, on the ground that History is, ideally at least, the highest form of literature.

> "Consider," he writes, "all that lies in that one word *Past*! What a pathetic, sacred, in every sense *poetic* meaning is implied in it; a meaning growing ever the clearer, the farther we recede in Time—the *more* of that same Past we have to look through! On which ground indeed must Sauerteig have built, not without plausibility, in that strange thesis of his: That History, after all, is the true poetry; that Reality if rightly interpreted, is grander than Fiction; nay that even in the right interpretation of Reality and History does genuine Poetry lie."

This doctrine of "Sauerteig's"—that is of Carlyle himself in one of his moods—expresses succinctly his attitude to History as potentially the highest of all the Arts. History, if it could find its Shakespeare, would be even more moving than poetry and drama, because of the added emotion aroused in the contemplation of events that we believe to have really occurred, of persons whom we believe to have really existed. Shakespeare's record of Hamlet has more value than any record of Cromwell that we are like to have, because Shakespeare wrote it; but if we believed Shakespeare's Hamlet to have really existed, our interest would be greater still. Indeed, to enhance our pleasure, there are moments when we half pretend to ourselves that Hamlet was a real person. Bradley's noble study of Shakespeare's Tragedies is based on such a pretence. Bradley leads us on to think of Hamlet as having a continuous life of his own between the scenes, and in the years before the play begins, the years when he rode on Yorick's back and studied at Wittenberg, although actually we know that there was "no such matter." For the same reason some people dislike confessing that fairies do not exist, just because the pleasures of fantasy would be greater if reality could be added to them.

Our interest in Cromwell derives from our belief that once there actually was such a man in England. The poetry of history lies in its being true. But Shakespeare's imagination refuses to be bound in the trammels of actual fact, so he refused to be an Historian. History, therefore, has not yet got its Shakespeare, and therefore Clio is still only in the second line of the Muses. But at least it has had its Carlyle, and that at least is something.

In his Essay on Boswell's *Johnson*, Carlyle illustrates very fully his emotional feeling towards History and the Past. History, he says in effect, has a two-fold emotional advantage over fiction: first, it is true, it really happened. These dear dead men and women were once alive and actual as we are now for *our* brief day. Secondly, the event described is in the *past*, and therefore has about it a special romance or pathos, to Carlyle at least and to most, I suppose, of those who like myself are specially devoted to History.

> "Thus," he writes, "for *Boswell's Life of Johnson* has Time done, is Time still doing, what no ornament of Art or Artifice could have done for it. Rough Samuel and sleek wheedling James *were* and *are not*. Their Life and whole personal Environment has melted into air. The Mitre Tavern still stands in Fleet Street: but where now is its scot-and-lot paying, beef-and-ale loving, cocked-hatted, pot-bellied Landlord; its rosy-faced, assiduous Landlady, with all her shining brasspans, waxed tables, well-filled larder-shelves; her cooks and bootjacks, and errand-boys, and watery-mouthed hangers-on? Gone! Gone! The becking Waiter who, with wreathèd smiles, was wont to spread for Samuel and Bozzy their supper of the gods has long since pocketed his last sixpence; and vanished, sixpences and all, like a ghost at cock-crowing. The Bottles they drank out of are all broken, the Chairs they sat on all rotted and burnt; the very Knives and Forks they ate with have rusted to the heart, and become brown, oxide of iron, and mingled with the indiscriminate clay. All, all has vanished; in every deed and truth, like that baseless fabric of Prospero's air-vision. Of the Mitre Tavern nothing but the bare walls remain there: of London, of England, of the World, nothing but the bare walls remain; and these also decaying (were they of adamant), only slower. The mysterious River of Existence rushes on: a new Billow thereof has arrived, and lashes wildly as ever round the old embankments; but the former Billow, with *its* loud, mad eddyings, where is it? Where? Now this Book of Boswell's, this is precisely a revocation of the edict of Destiny; so that Time shall not utterly, not so soon by several centuries, have dominion over us. A little row of Naphtha-lamps, with its line of Naphtha-light, burns clear and holy through the dead Night of the Past: they who are gone are still here; though hidden they are revealed, though dead they yet speak. There it shines, that little miraculously lamplit Pathway; shedding its feebler and feebler twilight into the boundless dark

> Oblivion—for all that our Johnson *touched* has become illuminated for us: on which miraculous little Pathway we can still travel, and see wonders."
>
> "Which of us but remembers, as one of the sunny spots in his existence, the day when he opened these airy volumes, fascinating him by a true natural magic! It was as if the curtains of the past were drawn aside, and we looked mysteriously into a kindred country, where dwelt our Fathers; inexpressibly dear to us, but which had seemed forever hidden from our eyes. For the dead Night had engulfed it; all was gone, vanished as if it had not been. Nevertheless, wondrously given back to us, there once more it lay; all bright, lucid, blooming; a little island of Creation amid the circumambient Void. There it still lies; like a thing stationary, imperishable, over which changeful Time were now accumulating itself in vain, and could not, any longer, harm it, or hide it."

Or again, at the end of the same volume of Essays he thus expresses the same emotional attitude to the Past from a larger field of vision:—

> "Consider History with the beginnings of it stretching dimly into the remote Time; emerging darkly out of the mysterious Eternity; the ends of it enveloping *us* at this hour, whereof we at this hour, both as actors and relators, form part. In essence and significance it has been called the true Epic Poem and universal Divine Scripture, *whose* 'plenary inspiration' no man, out of Bedlam, or in it, shall bring in question."

In *Past and Present* the account of Abbot Samson and the monastery of St. Edmondsbury is filled from beginning to end with this same sense of the mystery and poetry of the Past, once as actual as the Present, now vanished like a dream. And in particular Carlyle writes poetical dithyrambs on the *romance of the historical document*, which I would recommend to the notice of historical students. Monk Jocelin's chronicle is the document in question, sole base of Carlyle's biography of Abbot Samson.

> "It must be owned, the good Jocelin, spite of his beautiful child-like character, is but an altogether imperfect 'mirror' of these old-world things! The good man, he looks on us so clear and cheery and in his neighbourly soft-smiling eyes we see so well our *own* shadow, we have a longing always to cross-question him, to force from him an explanation of much. But no; Jocelin, though he talks with such clear familiarity, like a next-door neighbour, will not answer any question; that is the peculiarity of him, dead these six hundred and fifty years, and *quite deaf to us, though still so audible*! The good man, he cannot help it, nor can we."

At last Jocelin's historical document comes to sudden end, and with it our knowledge of the remaining course of Abbot Samson's heroic life. "The rest is silence." This is how Carlyle describes the matter:—

> "Magnanimous Samson, his life is but a labour and a journey; a bustling and a justling, till the still Night come. He is sent for again, over sea, to advise King Richard touching certain Peers of England, who had taken the Cross, but never followed it to Palestine; whom the Pope is enquiring after. The magnanimous Abbot makes preparation for departure; departs, and— And Jocelin's Boswellian Narrative, suddenly shorn-through by the scissors of Destiny, *ends*. There are no words more; but a *black line*, and *leaves of blank paper*. *Irremediable*: the miraculous hand, that held all this theatric-machinery, suddenly quits hold; impenetrable Time-Curtains rush down; in the mind's eye all is again dark, void; with loud dinning in the mind's ear, our real-phantasmagory of St. Edmundsbury plunges into the bosom of the Twelfth Century again, and all is over. Monks, Abbot, Hero-worship, Government, Obedience, Cœur-de-Lion and St. Edmund's Shrine, vanish like Mirza's Vision; and there is nothing left but a mutilated black Ruin amid green botanic expanses, and oxen, sheep and dilettanti pasturing in their places."

So far, for Carlyle's intense emotional feeling for the romance of History, the poetry of facts about the past, I have nothing but praise. His creed was that the poetry of History consisted in its being true, that the *fact* was poetic, because it was a fact and not a fiction about the Past. This feeling inspired at once the poetry and the realism of his historical narrative, whether in the *French Revolution*, in *Cromwell* or in *Frederic*. It made him worship the fact, the detail, the ascertainable truth, as the vehicle of poetry. Having been at pains to discover the fact, he was equally at pains to present it to the reader at once realistically and poetically. History is often written in a manner that is neither realistic nor poetical, and very seldom in a way that is both at once. That, I conceive, was Carlyle's great merit as an historian; he combines the poetry and the realism of history, so that they cannot be distinguished one from another.

But now I come to his great demerit as an historian. His fault was that he so often thought that there was only one side to a question. This habit of mind grew on him as he grew older, and as his doctrine of Hero-worship more and more dominated his interpretation of life. It is least in evidence—indeed it is not in evidence at all—in *The French Revolution*, which was his earliest historical work, and was moreover, most luckily, a story without a hero. The hero-worship and one-sidedness begins to be visible in his next historical works, the lectures on *Heroes* and *Past and Present*. To Carlyle nothing that Abbot Samson does can be wrong, no quarrel he espouses can have two sides. His next work, *Cromwell*, is the turning-point. There the theory comes out full-blown, and in

Frederic it governs everything, putting blinkers on to his judgment of men and things.

Carlyle's hero-worship was connected with a strongly felt emotional attitude to the Universe, which he had not really thought out intellectually, namely, his belief that evil or sham always come to grief in the end, and that right is always ultimately might and destined to triumph. Now in reality the connection between right and might is occasional only. It is not a universal law, traceable in all history. By Carlyle's own standards of judgment it can be shown that bad sometimes triumphs permanently over good, that "shams" often outlast "realities." If, as Carlyle thought, the French *ancien régime* perished because it was a "sham," French Protestantism had perished even sooner, and Carlyle would not have called that a "sham." Sometimes, indeed, Carlyle recognises that the Universe does not in fact move by the laws he had laid down for it, any more than by the Thirty-Nine Articles which he despised so much. The spleen of his later years was due, not only to indigestion, but to perpetually recurring doubt whether in fact God was asserting Himself in the world's affairs. But very often Carlyle will try to make his theory work in hell's despite, by distorting or overlooking some of the most important happenings of History.

I will take one small example, at random, out of *Past and Present.* In describing the martyrdom of St. Edmund by the Danes he very naturally takes the side of St. Edmund. But because he takes that side, he therefore has not only no good word to say for the Danes, whom he treats as mere harmful plunderers, but he travesties the facts of History by saying that "the earth was cleared of them." But in fact the earth was not cleared of them. They conquered and settled East Anglia and St. Edmund's own county, turning it into the Dane Law. English earth has not, I am glad to say, been cleared of them to this day. It has indeed been cleared of their heathenism and of their plundering habits, and St. Edmund's Christianity and love of order have triumphed. But the Danes are with us still; they are flesh of our flesh and bone of our bone. Moreover the Danes had something of value to give to the crass Saxon England of that time—valour, energy, independence of spirit, and seafaring and commercial habits. These things Carlyle might so easily have perceived. Indeed one feels certain that if, instead of modernising a monastic chronicle, he had happened to be writing, as he so easily might, a history of the Norse peoples taken from the Eddas and Sagas, in the spirit of his lecture on Odin, he would with peculiar gusto have described the energies and brave adventurous sea-farings and land-farings of the Danes, come as God's instruments to shake the purblind, dormant Saxon Kingdoms once more into active life. Carlyle is, in fact, so often the most partial, the most partisan of historians, sometimes on principle, as in the case of Cromwell, sometimes, as in this case, according to a whim or circumstance of the moment.

Yet not always. Nothing for instance could be more impartial and more profound than his remarks on the struggle between Church and State in the time of Anselm and of Becket in the chapter on Aristocracies, near the end of *Past and Present*, pp. 212–13 of the common edition. And above all we have the unalloyed humanity, sympathy and impartiality of his *French Revolution*. In that great work, Carlyle's sympathies are freely divided up among all the parties and actors, judging each man by his merits as a human being and not otherwise. "Cavaignac is angry with me," he writes, "for my treatment of the sea-green man and *impartialité* generally. I take no side in the matter. How very singular!" When he thus allows himself to be a spectator and not himself a combatant, how deep then is his penetration into the ways of men. I think *his* book is still the *best* book on the French Revolution.

I am well aware of all that it is not. It gives no account of the social causes of the French Revolution, no analysis of the *ancien régime*. Writing in 1835, he had not the help, so familiar to us, of de Tocqueville or of Taine, or of any of the later students of the old form of French life. His book is a narrative of the events of the Revolution, not of its causes. Moreover the narrative, written within forty-five years of the events themselves, is based on very few, and in many cases not very accurate, authorities. His details are, therefore, frequently wrong. Oscar Browning corrected him over the Flight to Varennes. Yet, with all these disadvantages, I know of no other book in French or English which conveys to me so living and, as I believe, truthful a picture of the moods of French men and women in those years when popular moods were the guide and impulse of events, because all institutions, authorities and customs had been for awhile abolished. Carlyle alone of those historians of the Revolution which I have read, gives the human psychology of the mob its proper place in the story of 1789–95. After the "whiff of grape-shot" in Vendémiaire, when France again got a government that could disregard the wishes of the Sections of Paris, the psychology of the mob falls back into its usual limited place in human affairs once more. But between 1789 and 1795 the variable sentiments of French men and women at large, and of the mob of Paris in particular, decided one crisis after another. How this happened seems to me more clearly written in Carlyle than in Acton, Aulard, Lenostre or any other of the best writers on the Revolution, who with more knowledge of documents had less understanding of mankind than the Carlyle of 1835. The Carlyle of later years allowed his theories and his dyspepsia largely to overcloud the shrewd, broad human sympathies and perceptions that dictated his *French Revolution*. The fact that he is neither for nor against the Revolution, and that he found no "hero" in it to worship, helped the soundness of his judgment in this book. I do not think that all our later learning has enabled us to produce any better analysis of the psychological causes of the

Terror than this of Carlyle. Here is Carlyle's explanation of it—and I know none better :—

> "Imagine, O Reader, that the Millennium were struggling on the threshold, and yet not so much as groceries could be had—owing to Traitors. With what impetus would a man strike Traitors in that case! Ah, thou canst not imagine it; thou hast thy groceries safe in the shops, and little or no hope of a Millennium coming!"

Or, again, his explanation of why the French people executed poor helpless, innocent Louis XVI., though ever so good a Republican as Tom Paine cried to them to spare him :—

> "But, on the whole, we will remark here that this business of Louis looks altogether different now, as seen over Seas and at the distance of forty-four years, from what it looked then, in France, and struggling confused all round one. For indeed it is a most lying thing that same Past Tense always; so beautiful, sad, almost Elysian-sacred, 'in the moonlight of Memory,' it seems; and *seems* only. For observe, always one most important element is surreptitiously (we not noticing it) withdrawn from the Past Time: the haggard element of Fear! Not *there* does Fear dwell, nor Uncertainty, nor Anxiety; but it dwells *here*; haunting us, tracking us, running like an accursed ground-discord through all the music-tones of our Existence: making the Tense a mere Present one! Just so is it with this of Louis. Why smite the fallen? asks Magnanimity, out of danger now. He is fallen so low this once-high man; no criminal nor traitor, how far from it; but the unhappiest of Human Solecisms: whom if abstract Justice had to pronounce upon, she might well become concrete Pity, and pronounce only sobs and dismissal!
>
> "So argues retrospective Magnanimity: but Pusillanimity present, prospective? Reader, thou hast never lived, for months, under the rustle of Prussian gallows-ropes; never wert thou portion of a National Sahara-waltz, Twenty-five millions running distracted to fight Brunswick! Knights Errant themselves, when they conquered Giants, usually slew the Giants: quarter was only for other Knights Errant, who knew courtesy and the laws of battle. The French Nation, in simultaneous, desperate dead-pull, and as if by miracle of madness, has pulled down the most dread Goliath, huge with the growth of ten centuries; and cannot believe, though his giant bulk, covering acres, lies prostrate, bound with peg and packthread, that he will not rise again, man-devouring; that the victory is not partly a dream. Terror has its scepticism; miraculous victory its rage of vengeance."

As to Carlyle's attitude to the whole Revolution, it is, as I say, no partisan attitude, but something much deeper than that. I know of nothing ever said about the Revolution better than this :—

> "The Fireship is old France, the old French Form of Life; her crew a generation of men. Wild are their cries and their ragings there, like spirits tormented in that flame. But, on the

> whole, are they not gone, O Reader? Their Fireship and they, frightening the world, have sailed away; its flames and its thunders quite away, into the Deep of Time. One thing therefore History will do, pity them all; for it went hard with them all."

If History contained more writing like that, Clio would not be the Cinderella of the Muses, uncertain at times whether she be indeed a Muse or only a kitchen-maid.

Again, Carlyle's insight into the character of particular actors (none of them fortunately judged by him to be "heroes") is so profound, so humorous, and touched in with strokes of inimitable art. Take this portrait of Sieyes which I have heard Sieyes' English biographer praise—(II. 5). The scene is the National Assembly, autumn of 1783, engaged on making the Constitution :—

> "With ineffable serenity sniffs great Sieyes, aloft, alone; his Constitution ye may babble over, ye may mar, but can by no possibility mend: is not Polity a science he has exhausted?"

Or this scene of an interview between Danton and Robespierre early in 1794 :—

> "One conceives easily the deep mutual incompatibility that divided these two: with what terror of feminine hatred the poor sea-green Formula looked at the monstrous colossal Reality, and grew greener to behold him;—the Reality, again, struggling to think no ill of a chief-product of the Revolution; yet feeling that such chief-product was little other than a chief-windbag, blown large by popular air."

Indeed, one great quality requisite for the proper treatment of history Carlyle has got in a superlative degree—humour. Many of us see the humour of events in History, not least in many episodes of the French Revolution, but we find it hard to give it verbal or literary expression. The humorous Englishman, smiling silently to himself over his pipe, can so seldom express in words the fun within him. But take Carlyle's account of Anacharsis Clootz' introduction of the "Deputation from the *genre Humain*" to the much flattered Assembly, which granted the world's representatives the "*honneur de la séance*"—(II. id.) :—

> "A long-flowing Turk, for rejoinder, bows with Eastern solemnity, and utters articulate sounds; but owing to his imperfect knowledge of the French dialect, his words are like spilt water, the thought he had in him remains conjectural to this day."

Or, in the rising of Prairial, when the Sections for the last time burst in on the Legislature—(III. 261) :—

> "One man we discern bawling 'for the space of an hour at all intervals,' '*Je demande l'arrestation des coquins et des lâches.*' Really one of the most comprehensive Petitions ever put up;

> which, indeed, to this hour, includes all that you can reasonably ask Constitution of the Year One. *I also demand arrestment of the Knaves and Dastards*, and nothing more whatever."

Or hear the cry with which he ends his account of the massacre of the Swiss Guards by the mob of August the Tenth, the laugh, as so often with him, choked in a sob :—

> "*O Peuple*, envy of the Universe! People, in mad Gaelic effervescence."

Cromwell, completed in 1845, appeared almost ten years after the *French Revolution*. Though both are written in Carlylese, there is little else in common between the two books in historical method. The earlier work was a popular presentment of a great theme, based indeed on the study of first-hand authorities of a sort, but on first-hand authorities too few and too inadequate to give to his work any value as a safe reference book for details. *Cromwell*, on the other hand, was the first serious study of Oliver's letters and speeches, and the result was a permanent addition to historical knowledge on a subject of the very first historical consequence, namely, the character of Cromwell, and to a less extent the character of the motives of the Puritan Revolution. Heretofore the actions of the Long Parliament and of the Roundhead Party, in and after the Civil War, had been interpreted on its political and constitutional side, with little reference to the religious issues, because those had ceased to interest the men of the eighteenth and early nineteenth centuries. Carlyle, from the documents, sympathetically resuscitated the real religious motives, long buried under the dust of centuries. So too, Cromwell had been regarded as a hypocrite. The Tories had no word too bad for him ; the Whigs dared not whole-heartedly defend a regicide, and moreover were puzzled by his religious zeal. The Dissenters, who might have understood him at least in part, had been thoughtfully excluded by the Legislature from the higher education of the country. And so no historian had ever studied Cromwell with an eye for his real motives. Carlyle's book changed this once and for all. Henceforth, as Froude said, Cromwell "may be loved or he may be hated, as he was loved and hated in his own time ; but we shall love or hate the man himself, not a shadow or a caricature of a man."

This great and permanent change in the opinion both of historical scholars and of the man in the street was effected by Carlyle, because he consented on this occasion to harness his Pegasus to the work of serious historical research. He became, in fact, his own Dry-as-Dust. Pegasus remains Pegasus and flies, but this time he drags a solid cart through the empyrean. I would not say that *Cromwell* is either a greater or a less achievement than the *French Revolution*. Only it is a totally different one. It is different in two respects. First, it is based on far more solid research; it even sacrifices its literary form and much even of its literary success to reproducing the letters and speeches *in extenso*. Secondly, though it taught the world much that was new and of supreme importance about Cromwell, it had not the impartiality and large charity of the *French Revolution*. Carlyle's *Cromwell* is dominated, for evil as well as for good, by the idea of *Hero-worship*, which is totally absent in the earlier work. Everyone who opposes Cromwell is written down as fool or knave—at best as "blockhead" like "wooden Bulstrode." Now, since almost everyone of note in England, except Milton and Blake, at one time or another opposed Cromwell, the result is scarcely fair to the nation as a whole. It is, in fact, not a national epic but a biography. Yet Carlyle's idea was originally to write a National Epic of Puritan England, in the style of his *French Revolution*. The rough sketch of several of its chapters was posthumously published under the title of *Historical Sketches*—a work well worth more attention than it receives, both for its own sake and as showing by what stages he arrived at the conception of his *Cromwell*.

The abandonment of this idea of a National Epic in purely literary form, and the substitution of a Biography in a form very far from purely literary, is a turning-point in Carlyle's career as an historian. It gave him a second wind, a new source of power, as Dry-as-Dust, as researcher, thereby increasing his historical importance somewhat at the expense of the literary pleasure given by his works. In the second place it meant the final triumph of his idea of Hero-worship as the main element in his interpretation of History. *Frederic*, which became the task, one might say the drudgery, of his later years, repeats and emphasises both these characteristics—intense application to detailed research in documents, and the subjection of historical impartiality to hero-worship. This simultaneous double change in the character of Carlyle's work is a reminder that depth of research does not necessarily increase impartiality of mind or breadth of sympathy.

If the transition stage from the *French Revolution* to *Frederic* can be seen in *Cromwell*, the transition from the *French Revolution* to *Cromwell* can be seen in *Past and Present*. In *Past and Present* he tasted the joys of first-hand work on an ancient document, Jocelin's Chronicle, as his sole raw material of his story. And in telling the tale of Abbot Samson he became sensitive to the pleasures of biography and to the allurements of Hero-worship. He wrote *Past and Present* in 1842 as a Parergon in the middle of his Puritan-Cromwellian studies, and we can see the effect of his experience with Jocelin upon those studies. Almost immediately after he had published *Past and Present*, he threw his unfinished National Epic of Puritanism into the back of a drawer and commenced to edit *Cromwell's Letters and Speeches*. One can, therefore, hardly doubt that his intellectual experience with Jocelin's Chronicle and Abbot

Samson affected his decision in favour of the biographic form, and the great place given to documents in the new work.

Well, to sum it all up, I believe that Carlyle's historical works are great history and great literature, although they are as unlike other histories as they are unlike other works of literature. He is one of the great eccentrics; he stands apart from any other school, like Browning or Meredith. Among historians he is, perhaps, the only great eccentric. Therefore, not everyone even among the most intelligent part of the public will care to read him. Not a few of you here to-night may have been out of sympathy with all I have said, because Carlyle's style does not appeal to you. But there will always be some to whom it does appeal. And those, the Carlyleans of every age, will, I expect, care more as time goes on for his histories, and relatively less for his philosophy and for his politics. His political and social views concerned his age more than ours; they had their effect, they served their generation, but they are now mainly curiosities of literature. His philosophy was the response of one very remarkable mind and heart to the mystery of the Universe. It produced some noble poetry, much of it collected in *Sartor*, and scattered throughout his other writings and in his letters and journals. But the greatest mass of solid and continuous good writing on subjects of permanent interest to all succeeding ages will, I believe, be found in his historical work.

[G. M. T.]

WEEKLY EVENING MEETING,

Friday, June 10, 1927.

Sir Arthur Keith, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

C. Leonard Woolley.

The Excavations at Ur.

[Abstract.]

The excavations started last year on October 28th and were closed down on the 19th of February. With the exception of Father Burrows, who replaced Dr. Legrain as epigraphist, my staff was the same as in the former season, Mrs. Keeling being responsible for the drawings, Mr. Whitburn for the architectural side, and Mr. Mallowan acting as general archæological assistant; as foreman I had again Hamoudi from Jerablus, while his two sons were engaged, one as photographer, one as junior foreman; the number of workmen employed was smaller than usual, in view of the nature of our programme, and averaged about 130. The work fell into two distinct parts, the excavation of building sites, principally the houses of the Larsa period, and the examination of a large prehistoric cemetery.

The house site lay just outside the sacred Temenos, against its S.W. wall. Here we cleared parts of five streets, laying bare the remains, for the most part very well preserved, of fifteen private houses, enough to give us a fair idea of town planning in the period between 2100 and 1900 B.C. Contrary to what excavations on other sites of later date had led us to expect, the houses proved to be remarkably well-built structures in burnt and crude brick, standing two storeys high, with an average for a moderate-sized house of twelve rooms. Of course the exigencies of ground space and of the owner's means introduced all sorts of variations, but throughout there was recognisable one general type. The house was built round a central courtyard on to which opened all the rooms; round this ran a wooden gallery reached by a staircase of brick below and of wood for the upper flight, giving access to the chambers of the top floor; the house roof extended over the gallery, sloping slightly inwards, and left in the centre a comparatively small opening which served as a light-well for the ground floor; gutters round the edge of the opening led the rain water from the roof into a drain in the middle of the brick-paved court. It was possible to identify some of the rooms. The front door led into a small lobby communicating with the court; facing the entrance was the reception-room, on one side the kitchen, and below the stairs the lavatory, with a drain exactly like that of a modern Arab latrine. Altogether the arrangement and appearance of the house was precisely what one finds in the better quarters of Basra or Baghdad at the present day.

We knew already that it was the custom to bury the dead under the floors of the rooms in which they had lived; this year we found an extension of the custom in that some of the houses were provided with a regular funerary chapel designed as part of the original building; the chapel was a long chamber, having at one end a brick altar and a column of brickwork (the meaning of this is uncertain), and below the pavement of the "nave" there was a vaulted brick tomb which acted as a family vault. some containing as many as ten bodies interred at different times. In one case, instead of the tomb, there were grouped round the altar clay bowls and coffins containing the bodies of infants, over thirty in number; it would look as if the chapel were dedicated to some goddess who was a patroness of children.

One of the most important discoveries made in the houses was that of a fallen brick arch in one of the doorways. This takes back the history of the arch as a feature of domestic architecture by several hundred years, and enables us to work out re-constructions of buildings of this period (c. 2100 B.C.) in a manner for which there was before no justification in fact. We can now be sure that the arch and the vault were regularly employed in temples, and it is not unreasonable to suppose, in view of certain peculiarities of ground-plan, that in the next great building period, that of 1400 B.C., the dome also was used.

The chief "finds" made in the houses were of inscribed tablets. Many of these were ordinary business documents, but we have besides a few religious texts, lists of the pious foundations of kings, grammatical texts and syllabaries, and mathematical tablets with such things as tables of square and cube roots, exercises in land measuration, etc.

Work on building sites other than that of the Larsa houses threw much new light on the topography of the sacred Temenos, filling up what had been blanks on our general plan and leading to the identification of the great temple of the Moon God Nannar, in the time of Nebuchadnezzar. The discovery of a remarkable building set up by Sin-idinnam, King of Larsa (c. 2000 B.C.), about a mile to the east of the Temenos enclosure showed how widely scattered were the public buildings of the city, and how much work remains to be done even after the excavation of the Temenos itself is complete. Further digging beneath the floors of the temple E-nun-makh, first excavated in 1922, brought to light new historical information and, amongst other objects, a set of ivory toilet instruments of fine Phœnician work and an ivory plaque bearing a remarkable Phœnician inscription.

The second part of our season was spent in the examination of a cemetery lying at the S.E. end of the Temenos and inside its later walls. The graves, distinguished by their levels and contents, belonged to three periods, the uppermost to 2800–2600 B.C., the next to about 3100 B.C., the earliest to some four hundred years before that. The upper levels were dated by inscribed cylinder seals, two graves of the first series belonging to members of the household of the daughter of King Sargon of Akkad, while from the second series came the seal of the wife of Mesannipadda, first King of the First Dynasty of Ur; the date of the lowest graves was deduced from the very great amount of deposit separating them from the middle series, and from the difference of script on the inscribed objects, this being semi-pictographic instead of fully developed cuneiform. In the lowest stratum the bodies are sometimes partly burnt, evidently a survival of an older practice of cremation; all signs of this disappear in the upper levels. The normal type of grave is a mere hole in the ground lined with matting, in which the body was placed wrapped in a mat; the offerings were grouped about it, and the whole covered with a second layer of matting before the earth was replaced. Later wickerwork coffins might take the place of the simple mats, and two or three wooden coffins were found. From the earliest times clay coffins were occasionally used, and these became more common as time went on; it is curious that the clay coffins, which would seem to be the more costly form of burial, were in fact the poorest in their furniture.

The objects from the graves were such as no previous excavations in Mesopotamia have produced, and it was noteworthy that in richness, in quality and in technique, they were better in proportion as they went back earlier in time. Though we have touched the oldest dateable strata yet found (I except that of the painted pottery, about which virtually nothing is known) it is clear that we have to deal with a civilisation which if not already decadent had at least been in existence for many centuries.

Most striking are the gold objects. The wealth of precious metal found in the graves is proof of the prosperity of the age, but also it is in these things that we have best evidence of its high state of culture, for the gold was the material on which the workman lavished most skill, and it alone has suffered no decay. Not only beads, pendants and personal ornaments are of gold, but we find even tools and weapons of gold or of electrum; these must have been ceremonial rather than for practical use, but they reproduce the forms of implements in copper and in silver. Far the finest is a dagger of gold and lapis-lazuli, in a sheath elaborately decorated with filigree; besides this we have a spear-head, an adze and two chisels of electrum; the handles of such weapons were of wood covered with gesso, painted red and bound with thin gold. Toilet utensils were made of the same metal—one set is in a finely decorated case—as well as in silver and copper; bowls, vases and lamps were sometimes of silver, and one silver bowl is beautifully fluted and engraved. Engraving on shell was fairly common: a set of plaques thus worked with animal subjects and framed in lapis-lazuli and red stone was the most attractive, while a gaming-board with inlaid geometrical designs is of perhaps greater interest, though of less artistic merit; a larger plaque showing a priest at sacrifice is an unusually fine example of the same technique. Ostrich eggs and sea shells were sometimes encrusted with lapis, mother-o'-pearl and red paste. Stone vases in "alabaster," steatite and limestone were very numerous, copper was astonishingly abundant, and we found one instrument of iron, this from a grave in the lowest stratum. The beads are of lapis and carnelian, very well cut and polished, and were set with others of silver and gold, the latter being often made with a thin plating over copper. Only the pottery is disappointing in its quality. Sculpture is represented by some curious figures in unbaked clay and by a fragmentary but most important relief showing an empty chariot drawn by lions being led in procession, perhaps in the funeral procession of a king. Lastly we have recovered a great number of engraved cylinder seals, invaluable for the study of early art forms. Altogether the season's work has produced a mass of material, much of it entirely novel, whose importance for the early history of Mesopotamia it would be difficult to exaggerate. I am glad to say that there is every reason to believe that discoveries of no less importance await us next winter.

[C. L. W.]

GENERAL MONTHLY MEETING,

Monday, June 13, 1927.

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Sir Robert Waley Cohen, K.B.E.
Thomas Holt Hutcheson,
Miss Margaret Preston Lewis, B.Sc.
Colonel Thomas Herbert Minshull, R.E. D.S.O.
Charles Egerton Mott,

were elected Members.

The Special Thanks of the Members were returned to Mrs. C. H. Baden-Powell for her gift of Iceland Spar, New Zealand Jade and Quartz, formerly belonging to the late James Farmer, M.R.I.

The following PRESENTS received since the last meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

British Electrical Industries Research Association—Dielectric Phenomena: Electrical Discharges in Gases. By S. Whitehead. 8vo. 1927.
Church, Arthur H., Esq., M.A. D.Sc. F.R.S. (the Author)—The Building of an Autotrophic Flagellate. (Oxford Botanical Memoirs, No. 1.) 8vo. 1919.
Crozet-Fourneyron et Cie.—Invention de la Turbine. By Marcel Crozet-Fourneyron. 8vo. 1927.

GENERAL MONTHLY MEETING,

Monday, July 4, 1927.

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Major Frederick Arthur Freeth, O.B.E. D.Sc. F.R.S.
Mrs. Graham Rees-Mogg.

were elected Members.

The Managers reported that they had received the Balance (£37 10*s.*) of the Legacy of the late C. E. Layton, M.R.I.

The following Presents received since the last meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

Parker, W. Rushton, Esq., M.D. M.R.I. — An Egyptian Hieroglyphic Dictionary. By Sir E. A. Wallis Budge. 4to. 1920.
Egyptian Grammar. By A. H. Gardiner. 8vo. 1927.
Ancient Records of Egypt. By J. H. Breasted. 5 vols. 8vo. 1906-7.
Ancient Records of Assyria and Babylonia. By D. D. Luckenbill. 2 vols. 8vo. 1926.

LONDON: PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
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OF THE
Royal Institution of Great Britain

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21 ALBEMARLE STREET, LONDON, W.1

October 1928

ROYAL INSTITUTION OF GREAT BRITAIN.

GENERAL MONTHLY MEETING,

Monday, November 7, 1927,

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Harold Bright, M.A.
Hubert Alexander Gill, M.A.
Arthur Henry Jenkin,
Mrs. Anne Patrick,
Edward Blair Robertson,
Maurice Ruffer,

were elected Members.

The Chairman reported the death of Professor Mittag-Leffler on July 12, of Professor Svante Arrhenius on October 2, and of Lord Iveagh on October 7, and the following resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to place on record their sense of the great loss the Institution has sustained by the death of Gosta Magnus Mittag-Leffler, Professor of Mathematics in the University of Stockholm, Foreign Member of the Royal Society of London (1896), Honorary Member of the Royal Institution (1912), Founder and Editor-in-Chief of Acta Mathematica.

Professor Mittag-Leffler was an eminent Mathematician, and the originator of the Theorem of Functions which bears his name; he made many notable studies on pure mathematics, particularly one of special importance on the Theory of Linnear Differential Equations and their integration. He was the author of numerous valuable communications to the Acta Mathematica and other journals.

The Managers desire to express on behalf of the Members their sincere sympathy with the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Royal Institution has sustained by the death of Svante Arrhenius, Ph.D. (Upsala and Leipzig), Hon. Sc.D. (Oxon. and Cantab.), LL.D. (Birmingham), Professor of Physics, and Rector of the University of Stockholm, Director of the Physico-Chemical Department of the Nobel Institute, Foreign Member of the Royal Society, Honorary Fellow of the Chemical Society, Honorary Member of the Royal Institution, Member of the Academy of Sciences, Paris, National Academy of Washington, and numerous other Academies. Davy Medal (1908), Nobel Prize (Physics) (1903), Faraday Medal (1914).

Professor Arrhenius was a brilliant investigator in the field of Physical Chemistry, and discovered the Theory of Electrolytic Dissociation, which has had far-reaching effects on all branches of Chemistry, leading to new methods of research, and the application of physico-chemical methods to the investigation of problems connected with the living organism. He was the author of the following books:—Electro Chemistry (1902); Lehrbuch der Kosmischen Physik (1903); Theories of Chemistry (1907); Immunochemistry (1907); Worlds in the Making (1908); Depths of the Universe (1909); Theory of Solutions (1912); Quantitative Laws in Biological Chemistry (1915); Destinies of the Stars (1918); Chemistry in Modern Life (1926); as well as numerous memoirs containing the results of his original investigations contributed to the Proceedings of many Academies and learned Societies.

Professor Arrhenius delivered two Friday Evening Discourses at the Royal Institution on June 3, 1904, on the Development of the Theory of Electrolytic Dissociation, and on June 9, 1911, on the Doctrine of Immunity; and two Courses of Lectures on Cosmogonical Questions, the Tyndall Lectures in 1909, and on Identity of Laws in General and Biological Chemistry in 1914.

The Managers desire to express on behalf of the Members their sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Institution has sustained by the death of the Earl of Iveagh, K.P. G.C.V.O. LL.D. (Dublin), F.R.S., Chancellor of the University of Dublin.

Lord Iveagh was a Member of the Royal Institution for twenty-five years, and always took a keen interest in its welfare and the promotion of the objects of the foundation. He rendered valuable help in the conduct of its affairs by his services on the Board of Managers for several years.

The Managers desire to express on behalf of the Members their sympathy with the family in their bereavement.

The Managers reported that they had received a Legacy of £4599 0*s.* 6*d.* from the late Sir William Phipson Beale, Bart., M.R.I.

The following Lecture Arrangements for November and December, 1927, were announced:—

Sir John Herbert Parsons, C.B.E. LL.D. F.R.S. F.R.C.S. Three Lectures on Light and Sight. On *Tuesdays*, Nov. 1, 8, 15. (The Tyndall Lectures.)

Sir William Bragg, K.B.E. LL.D. F.R.S. M.R.I., Fullerian Professor of Chemistry. Four Lectures on A Year's Work in X-Ray-Crystal Analysis. On *Tuesdays*, Nov. 22, 29, Dec. 6, 13.

H. Clifford Smith, M.A. F.S.A., Assistant Keeper, Victoria and Albert Museum. Two Lectures on The Furniture and Equipment of the Mediæval House. On *Thursdays*, Nov. 3, 10.

R. E. Mortimer Wheeler, M.C. M.A. D.Litt. F.S.A., Keeper and Secretary of the London Museum. Three Lectures on London before the Norman Conquest. On *Thursdays*, Nov. 17, 24, Dec. 1.

James Kewley, M.A. F.I.C. M.I.Chem.E. M.R.I. Two Lectures on Petroleum Natural Gases and their Derivatives. On *Thursdays*, Dec. 8, 15.

Emile Cammaerts, C.B.E. F.R.Hist.S., Chevalier de l'Ordre de Leopold. Two Lectures on The Main Features of Modern English Literature. On *Saturdays*, Nov. 5, 12.

GUSTAV HOLST. Three Lectures (with Musical Illustrations) on SAMUEL WESLEY AND ROBERT PEARSALL. On *Saturdays*, Nov. 19, 26, Dec. 3.

F. J. M. STRATTON, M.A. D.S.O. D.L., Fellow of Gonville and Caius College, University of Cambridge. Two Lectures on RECENT DEVELOPMENTS IN ASTROPHYSICS. On *Saturdays*, Dec. 10, 17.

PROFESSOR EDWARD N. DA C. ANDRADE, D.Sc. Ph.D. M.R.I., Professor of Physics, Military College of Science, Woolwich. Six Lectures (Illustrated by Models and Experiments), adapted to a Juvenile Auditory, on ENGINES: 1. THE RULES WHICH ALL ENGINES MUST OBEY; 2. LEARNING ABOUT STEAM; 3. ENGINES WHICH WORK TO AND FRO; 4. ENGINES WHICH WORK ROUND AND ROUND; 5. PUTTING THE FURNACE IN THE CYLINDER; 6. HEAT ENGINES WHICH PRODUCE COLD. On Dec. 29 (*Thursday*), Dec. 31 (*Saturday*), 1927; Jan. 3, 5, 7, 10, 1928. (102nd Course.)

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

The Astronomer Royal—Photograph of the Solar Eclipse. 1927.

Bartholomew, Mrs. Claude, M.R.I. (*the Author*)—The Cause of Evil. 8vo. 1927.

Boyle, Miss M. E. (*the Author*)—In Search of our Ancestors. 8vo. 1927.

Carnegie Institution, Washington—The Analytical Expression of the Results of the Theory of Space-Groups. By R. W. G. Wyckoff. 8vo. 1922.

Chicago Academy of Sciences—Flora of the Chicago Area. By H S. Pepoon. 8vo. 1927.

Gill, Frank, Esq., M.R.I.—Comité Consultatif International des Communications Téléphoniques: Plenary Session, 1926, and Supplement (English Edition). 2 vols. 4to. 1927.

Page, Calvin S. (*the Author*)—The Atomic Science. 8vo. 1927.

Parker, W. R., Esq., M.D. M.R.I.—Ancient Weights and Measures. By Sir Flinders Petrie. 4to. 1926.

Glass Stamps and Weights. By Sir Flinders Petrie. 4to. 1926.

Gurob. By G. Brunton and R. Engelbach. 4to. 1927.

Objects of Daily Use. By Sir W. M. Flinders Petrie. 4to. 1927. (British School of Archæology in Egypt Publications, Vols. XXXIX–XLII.).

Post Mortem: Mere Mortals. By C. MacLaurin. 2 vols. 8vo. 1925.

Rockefeller Institute for Medical Research—Reprints, Vols. LX–LXI. 8vo 1927.

Royal Society of London—Scientific Work of Spencer Pickering, F.R.S. By T. M. Lowry and Sir J. Russell, with biographical Notice by A. Harden. 8vo. 1927.

Safety in Mines Research Board—Publications, Vol. I, 1923–25. Reports relating to Research into Coal Dust, Fire Damp and other Sources of Danger. 8vo. 1927.

Taylor, F. Coston, Esq., M.A. M.R.I.—Catalogue of Pictures in the Marley Bequest, Fitzwilliam Museum, Cambridge. Compiled by W. G. Constable. 8vo. 1927.

Yale University Library—The Anatomy of Science. By G. N. Lewis. (Silliman Lectures.) 8vo. 1926.

China and the Occident. By G. N. Steiger. 8vo. 1927.

GENERAL MONTHLY MEETING,

Monday, December 5, 1927,

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

William Howard Barnes, B.Sc.
Rudolph David Blattner,
Miss Constance M. Demuth,
Eric Malcolm Fraser,
Mrs. F. A. Freeth,
Howard William Hammond,
Arthur Lister Harrison, J.P.
William Polk Jesse,
Eric Ivan Lloyd, M.A. M.B. F.R.C.S.
Mrs. Elizabeth Stevenson McWilliam,
Richard Wharton Marriott,
Alfred William Mond, B.A.
Henry Mond.
John Gibb Nicholson.
Roger North, B.A., LL.B.
Mrs. Laura Southard Riviere.
Frederick William Shurlock, B.A. B.Sc. F.Inst.P.
Leonard Heseltine Swinbank.
Geoffrey Ingram Taylor, M.A. F.R.S.

were elected Members.

The Special Thanks of the Members were returned to Sir Ernest Moon, K.C.B. K.C. LL.B. M.R.I., for his Donation of £500 to the Research Fund.

The following Lecture Arrangements Before Easter, 1928, were announced :—

PHILIP R. COURSEY, B.Sc. F.Inst.P. M.I.E.E. M.R.I. Two Lectures on THE DEVELOPMENT OF DIELECTRICS FOR ELECTRICAL CONDENSERS. On *Tuesdays*, Jan. 17, 24.

PROFESSOR ARTHUR PERCIVAL NEWTON, M.A. D.Lit. F.S.A., Rhodes Professor of Imperial History, University of London. Three Lectures on 1. THE MERCANTILE EMPIRE, 1606–1783; 2. THE SETTLEMENT OF THE DOMINIONS, 1783–1870; 3. THE DEPENDENT EMPIRE AND THE BRITISH COMMONWEALTH OF NATIONS, 1870–1926. On *Tuesdays*, Jan. 31, Feb. 7, 14.

PROFESSOR JULIAN S. HUXLEY, M.A., Fullerian Professor of Physiology. Six Lectures on THE BEHAVIOUR OF ANIMALS. On *Tuesday*, Feb. 21, *Wednesday*, Feb. 29, *Tuesdays*, March 6, 13, 20, 27.

PROFESSOR JOCELYN F. THORPE, C.B.E. D.Sc. F.R.S. M.R.I., Professor of Organic Chemistry, Imperial College of Science and Technology. Two Lectures on THE SIGNIFICANCE OF UNSATURATION IN CARBON COMPOUNDS. On *Thursdays*, Jan. 19, 26.

SIR WILLIAM BRAGG, K.B.E. D.Sc. F.R.S. M.R.I., Fullerian Professor of Chemistry. Three Lectures on FROM FARADAY'S NOTE BOOKS. On *Thursdays*, Feb. 2, 9, 16.

PROFESSOR F. LL. GRIFFITH, M.A. LL.D. F.B.A. F.S.A., Professor of Egyptology, University of Oxford. Two Lectures on NUBIA IN ANTIQUITY AND IN THE MIDDLE AGES. On *Thursdays*, Feb. 23, March 1.

J. J. FOX, O.B.E. D.Sc. F.I.C., The Government Laboratory. Two Lectures on OPTICS AND CHEMISTRY. On *Thursdays*, March 8, 15.

GROUP-CAPTAIN MARTIN FLACK, C.B.E. M.A. M.B., Director of Medical Research, Royal Air Force Medical Service. Two Lectures on THE PHYSIOLOGICAL ASPECTS OF FLYING. On *Thursdays*, March 22, 29.

PROFESSOR R. W. CHAMBERS, M.A. Litt. D., Professor of English Literature, University College, London. Two Lectures on SOME TUDOR BIOGRAPHERS. On *Saturdays*, Jan. 21, 28.

HENRY COPE COLLES, M.A. B.Mus. F.R.C.M., Musical Critic of "The Times." Three Lectures on MUSICAL LONDON FROM THE RESTORATION TO HANDEL (1660–1759): 1. INSTRUMENTAL MUSIC; 2. THEATRICAL MUSIC; 3. THE CANTATA AND THE ORATORIO. (With Musical Illustrations). On *Saturdays*, Feb. 4, 11, 18.

CAMPBELL DODGSON, C.B.E. M.A., Keeper of Prints and Drawings, British Museum. Two Lectures on THE LIFE AND WORK OF ALBRECHT DÜRER. In connection with the fourth Centenary of his death (April 6, 1528). On *Saturdays*, Feb. 25, March 3.

SIR ERNEST RUTHERFORD, O.M. LL.D. D.Sc. Pres.R.S. M.R.I., Professor of Natural Philosophy, R.I., and Cavendish Professor of Experimental Physics, University of Cambridge. Four Lectures on THE TRANSFORMATION OF MATTER. On *Saturdays*, March 10, 17, 24, 31.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Bragg, Sir William, K.B.E. D.Sc. F.R.S. M.R.I.—Set of Stereoscopic Photographs of Crystal Models, with Stereoscope.

Bryant, A. J., Esq., M.R.I.—System of Geography. By Bernhard Varenius. Improved and illustrated by Sir Isaac Newton and Dr. Jurin. 2 vols. 8vo. 1734.

California, University of—Egyptian Tomb Steles and Offering Stones. By H. F. Lutz. [Egyptian Archæology, Vol. IV.] 4to. 1927.

Colombia University, New York—Elementary Conditions of Human Variability. By R. Dodge (Ernest Kempton Adams Fund for Physical Research, Publication No. 10). 4to. 1927.

Woolwich Research Department—Under Secretary of State for War—Survey of Radiological Research in the Research Department, Woolwich. 8vo. 1927.

WEEKLY EVENING MEETING.

Friday, January 20, 1928.

SIR ARTHUR KEITH, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

SIR WILLIAM BRAGG, K.B.E. D.Sc. F.R.S. M.R.I.,
Fullerian Professor of Chemistry.

Photo-electricity.

THE relations between light and electricity are of unending interest. More than fifty years ago the experiments of Faraday and the calculations of Maxwell culminated in the splendid electro-magnetic theory of light, on which subsequent research has built largely and firmly.

Even in the early days of the theory there was known to exist a peculiar and special relation to which the name of photo-electricity was given. The theory gave no ready explanation of it. Light falling upon a metal plate could cause the plate to become positively charged; ultra violet behaved better in this respect than visible light. When subsequently the electron was recognised and defined, it became clear that the photo-electric effect could be explained as due to an emission of electrons from the metal under the influence of the light, and various suggestions were offered as to the nature of the mechanism. It was clear that the absorption of energy from the electro-magnetic waves of light was replaced by the energy of electrons in motion: though at that time there was no obvious relation between the quantities of energy in the two forms. As time went on it was realised that this particular transformation was not an isolated effect; it was but an instance of one of the most important and widespread processes of physics. It was to be observed not only in the case of light but of X-rays and γ-rays also; and indeed in their case it was infinitely plainer and more accessible to experiment. But it also became continually clearer that the process was not to be explained in any simple fashion, if at all, by the original electro-magnetic theory of light, or by any natural development of that theory.

At the same time a number of other physical phenomena, and especially the relation between the quality of the radiation from a hot body, and the temperature of that body, showed also that the older theories were unable to account for the facts. They could be accounted for by supposing that heat could be radiated or absorbed only in parcels of definite size, no fractions being allowed. If such an idea might be reconciled with the undulatory theory, i.e., of electromagnetic waves, at least it was not contained therein. This was the beginning of the quantum period.

In 1905 Einstein published a pioneer paper in the "Annalen der Physik" in which the quantum was given an identity even more definite than that which Planck had assigned to it in his studies on heat radiation. Light was to be regarded not as a uniform train of spherical waves but as a flight of "quanta," corpuscles of some form to be better known on closer acquaintance. By reverting to a corpuscular theory of light he was able to connect together the new facts, which were in such apparent discord with the undulatory theory. In doing so he, of course, shut his eyes for the moment to the marvellous and most successful development of the latter theory to the explanation of such phenomena as reflection, refraction and diffraction. He would simply find a new theory to connect together the new facts; this done, the new and old theories could be brought together in the hope of welding them together. As he announced in the title of his paper, he was going to take the heuristic point of view.

Since 1905 new discoveries of first-class importance have been made, but still there is no simple answer to the old question. The most recent and, so it is said, the most successful solution is given by the hypothesis of so-called Wave-Mechanics, due largely to M. L. de Broglie and Dr. E. Schrödinger. It is now the subject of eager discussion in mathematical and physical circles.

The Managers of the Royal Institution have invited Dr. Schrödinger to give a short course of lectures on Wave-Mechanics at the Royal Institution, and have been gratified by the acceptance of their invitation. It has been arranged that the lectures shall be given on March 5th, 7th, 12th and 14th, at 5.15 p.m. Also Professor Whittaker, of Edinburgh, has accepted an invitation to give the Friday Evening Discourse on March 16th, on which occasion he will try to summarise for the benefit of our Members the features of the new position.

My discourse this evening is meant to be of a preparatory nature. I would like to set out the observed facts for which it is so difficult to find a common explanation, so far as it is possible to do so in the short time at my disposal. I cannot, of course, tell the whole story; I can only describe a few of the most important details of it.

Einstein begins his 1905 paper by pointing out, as a curious fact, that matter is now looked on as a collection of discrete particles, while light, which resembles matter in being one of the great phenomena, is treated as a continuous distribution in the space over which it passes; and he suggests that it would be well to consider a discrete view of light also. He then enumerates certain experimental results which would be in obvious agreement with the new theory, though not with the old. Most of them are beyond our capacity to consider in the short hour of this discourse; but two of them may well be considered by us as amply sufficient to illustrate our argument and easily demonstrable even on the lecture table.

The first of them is summed up in what is called Stokes's Law of fluorescence. When light falls upon a fluorescent substance the fluorescent light evoked is always of longer wave-lengths than the exciting light; in other words, its frequency is less. Now, if a corpuscular theory of light is to be adopted, wave motion of a given frequency must be replaced by some characteristic of the corpuscle: and it appears that the two, and only two, characteristics of a wave-motion, namely, frequency and intensity, are to be replaced respectively by individual energy and numbers of corpuscles. This has to be done in order to explain both the results we are considering and a number that we are not. On the corpuscular theory a beam of violet light is a flight of corpuscles each having a certain energy which is proportional to the frequency of the light. A beam of green light is a flight of corpuscles each having a certain energy less than the other, because the frequency is less. It is easy to imagine that the flight of more energetic corpuscles may be degenerated by impact with the body into a flight of less energetic corpuscles. But it is very difficult to see how undulations of one wave-length can on meeting material substances be converted into undulations of a different wave-length.

A still more striking argument is found in the photo-electric effect itself. Lenard had shown three years before that when the electrons came out of the solid body under the influence of the light their individual energy did not depend upon the intensity of the light, but on its frequency; the intensity affected only their number. This was an extraordinary result. It might have been thought that the more energetic or violent the waves of light which caused the emission of electrons, the greater the velocity with which the electrons would start on their way. And as to the influence of frequency, it would be difficult to say how it would go; though one would not readily expect it to be the sole arbiter of the velocity. But if light was corpuscular the process, though still unexplained in detail, ceased to look so strange. Each corpuscle acting independently would cause the emission of an electron. The intensity of the beam of light would depend on the number of corpuscles it contained, and their impact on the body would cause a proportionately large emission of electrons, making the photo-electric effect. And light of high frequency would on the corpuscular view consist of swiftly moving corpuscles which, having large energy, would cause the emission of relatively swift electrons. The whole phenomenon, by its character and definiteness, spoke entirely for the corpuscular theory. Einstein explains the point perfectly clearly in his famous paper of 1905, and suggests that, if he is right, far more careful quantitative observations would be justified, as they would give an authoritative verdict on the validity of the theory which he proposed. He suggests that workers might bear his views in mind when proceeding with their experiments.

Einstein's theory must have been discussed exhaustively; but, so far as I know, no allusion to it, or criticism, is to be found in the principal journals of physics for some years afterwards. In fact, it was not until seven years later that Richardson and K. T. Compton carried out the careful measurements which Einstein suggested.

The results were in absolute accordance with the exact rules which he had supposed would govern the photo-electric effect, having based them on the less well-defined information which was available when he first wrote.

I must now be permitted to describe some experiments which I carried out in Australia twenty years ago, in conjunction with several helpers, in particular R. Kleeman and J. P. V. Madsen. I had been working out certain laws which governed the passage through matter of the X-rays emitted by radium; and I had tried to carry over the information so obtained to an investigation of the behaviour of the β and γ-rays. Now when γ-rays fall upon a substance they excite β-rays, which are electrons in very swift motion. In fact the analogy with the photo-electric effect is exact; for the rays as we now know are a form of light of very high frequency. The characteristic features of the photo-electric effect appear here also and greatly emphasised. The swift β-rays are far easier to detect and to examine than the electrons set in motion by light. The parallelism was not then so obvious as it is now, and for my own part I could not then believe in its existence. Our experiments fitted in perfectly with a corpuscular hypothesis of the nature of the γ-rays; and I supposed that the undulatory theory of light was unshakeable. I should, of course, have thought otherwise if I had been aware of Einstein's paper, to which I have already referred; but it is easy to miss a single reference when one is in a very isolated laboratory, and, as I said before, there are few if any allusions to the paper in the current literature of the years immediately following its appearance.

Our experiments in 1907 and 1908, coupled with others to which we were able to refer, led us to the following conclusions:—

(1) When β-rays or γ-rays were incident on matter the energy of any resulting secondary radiations of any kind, β or γ, came from the rays. There was no "induced" radioactivity; no case of the energy of an atom being tapped. The adoption of this hypothesis simplified all further considerations of the phenomena.

(2) The so-called hard or very penetrating γ-rays produced swift and penetrating β-rays; soft γ-rays produced slow β-rays.

(3) The penetrating power or quality of the β-rays, produced by the action of γ-rays, depended only on the quality of the γ-rays, not upon the intensity of the γ-rays nor

upon the nature of the substance in which the transformation of energy took place; it was practically the same as that of the β-rays issuing from the radium itself.

(4) The β-rays produced in any substance by the action of the γ-rays moved at the outset in the original direction of the γ-radiation.

The last statement is capable of simple demonstration in the lecture room. The necessary apparatus is shown in the figure, which is taken from a paper by Madsen and myself. The radium is buried in a mass of lead, at the foot of a conical pit. Both β- and γ-rays issue from the mouth of the cone, but the former are turned aside by a strong magnet. The γ-rays pass on alone into an ionisation chamber. The beam of γ-rays is not quite free of β-rays because the latter are aroused even in the air through which the γ-rays pass on their way to the chamber, but the magnet has removed most of them. If now a carbon plate be placed in the path of the γ-rays as they enter the chamber there is a large increase in the ionisation current; the γ-rays as they pass through the block on their way generate β-rays, which cause the observed effect. But if the carbon plate be placed on the far side of the chamber so that the γ-rays strike it on the way out, there is a relatively small increase in the current. Yet the rays must have generated β-rays to very nearly the same amount in both positions of the plate; and the dissymmetry between the intensities of the "emergence and incidence" β rays (the one is ten times that of the other) tells us that the β-rays are largely thrown forward as they are produced. In fact, from what is otherwise known of the behaviour of β-rays on their way through matter, we might suppose that the β-rays were thrown forward exactly in the line of the γ-rays, for the substance of the carbon screen would turn back a certain proportion of the rays, and the incidence radiation could be accounted for in this way. Nevertheless, we know now, from more accurate work which we shall presently consider, the β-rays are not so closely tied down to the forward movement, and when the γ-rays are soft and the β-rays relatively slow the difference between emergence and incidence is less marked. The experimental difference is also much less when the carbon plate is replaced by screens of higher atomic weight.

Thus again we have those characteristics which led Einstein to suggest a corpuscular theory of light in order to account for them when manifested in the photo-electric effect. Here they are displayed in an immensely enhanced degree. Consider the γ-ray as a spherical wave weakening as it spreads away from its source. It arrives at a certain atom; a β-ray springs out of the atom with an energy which is just the same, no matter how intense are the γ-rays, or how the intensity has faded with distance. Moreover, it possesses a forward momentum which could not possibly, by millions of times, be given to it by the wave, although a wave can impart a certain momentum to an encountered obstacle.

These questions of energy and momentum are even more obviously displayed in the case of the X-rays and the electrons which are concerned both in their production and their absorption. In the X-ray bulb the electron is driven with a certain speed against the anticathode, the X-ray which is thereby excited passes away and, falling on some substance, it may be far or near, sets in motion an electron which has a speed comparable with, sometimes nearly equal to, that of the original. Here again is the same kind of transformation of energy, which is simply pictured as carried, unchanging on its way, either by the β electron or the corpuscle of the γ-ray.

I may say, I think, that in these experiments we were, though unwittingly, carrying out Einstein's suggestion that the corpuscular hypothesis deserved careful exploration; and the results were in entire agreement with his hypothesis. It was true, however, that I thought of the X-ray and γ-ray problems as distinct from that of light; and I ventured prematurely to give form to the corpuscle as a neutron, an electron compensated as to its charge by the addition of a neutralising amount of positive.

In 1910 and 1911 C. T. R. Wilson published accounts of his experiments with his fog chamber. These rendered obvious to the eye those tracks of the various rays through a gas which we had endeavoured to map out by more indirect means, and entirely confirmed the conclusions that had been reached. I may perhaps be allowed to illustrate this in the special case of the α-rays, though they do not fall within the limits of our subject. In Fig. 1 is shown a set of tracks of α-rays drawn so as to be in accord in the first place with the work done in Adelaide; and also to agree with the more recent experiments on their scattering performed by Rutherford and Geiger. Fig. 2 is from a photograph of one of the well known early achievements of Wilson. It may be a matter of interest that on a certain occasion in the Cavendish Laboratory at Cambridge we showed them to each other for the first time.

Wilson's photographs of the tracks of X-rays, β-rays, and cathode rays, placed the argument on a different plane; and their beautiful detail opened up the way to quantitative measurements which had seemed impossible. It was now obvious to the eye that X-rays and γ-rays did not of themselves ionise a gas, but set in motion electrons which did so. The lengths of the tracks of these electrons in a gas could be measured, and their initial velocities be calculated therefrom; their directions of ejection could also be observed. All fitted in perfectly with the quantum theory of the X- and γ-rays, even when refinements could now be studied, as, for example, the differences in velocities of ejection which were to be expected on Bohr's theory of stationary state.

In 1913 the discovery of the diffraction of X-rays by the crystal opened up a new point of view, and once more changed, or rather enlarged, the aspect of the question. It now appeared that X-rays and γ-rays in these effects behaved like long trains of spreading waves, and no clear picture of what happened could be obtained in any other way. In this matter the corpuscular theory was at sea. Even the old single pulse theory was in no better case, because it did not provide the regular succession of similar waves which was required to account for the crystal effect. The new discovery consolidated the position, linking together light, X-rays and γ-rays as one phenomenon; if they were regarded from the point of view of the undulatory theory they were all waves which differed only in frequency. If any other point of view was taken, that of a corpuscular theory for example, the equivalence must be expressed in some equally effective fashion. On the other hand, the simplification emphasized the difficulties of the position, since it left no escape from the necessity of finding a theory which could with equal ease and effectiveness express itself in terms either of waves or of

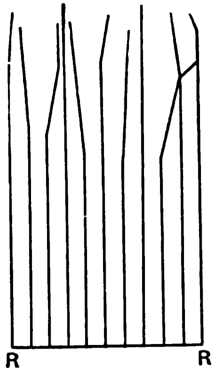

FIG. 1.

Suggested forms of the paths of α particles projected upwards from radium at R R.

(From "Studies in Radioactivity," by Sir W. H. Bragg. Courtesy of Macmillan & Co., Ltd.)

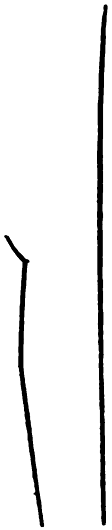

FIG. 2.

From photograph of α-ray tracks by C. T. R. Wilson.

corpuscular projectiles. Whether or no this has yet been done by any of the theories now in being is indeed the question of the day; our judgment will, I hope, be assisted by the lectures that are to follow.

Meanwhile matters have by no means remained stationary since the powers of X-ray and crystal analysis have been put into our hands. With the aid of the X-ray spectrometer we can measure with extreme accuracy the wave-lengths and frequencies of X-rays. When X-rays are incident upon a substance they are to some extent scattered as such, just as a ray of light is scattered in passing through a dusty atmosphere. This is apart from the photo-electric effect, namely the production of swift moving electrons at the expense of the energy of part of some of the incident rays. The X-rays are scattered by the atoms and electrons, the light being of coarser wave-lengths is scattered by the coarser particles of dust. Now when γ-rays, which are very fine X-rays, are scattered it has been often remarked that the scattered rays are somewhat softer than the originals. A. H. Compton used the spectrometer to examine if possible this softening, if X-rays could show it. They not only did so, but the experiments showed a real change of wave-length varying according to circumstances, and obeying simple rules. It appeared, in fact, that there was a definite small change in wave-length, which depended on the angle of scattering, but not on the original wave-length nor on the nature of the scattering material. Figs. 3 and 4, taken from Compton's new book "X-Rays and Electrons,"* show the effect very clearly. They represent the results of the spectrometer observations; in each curve one hump is due to rays that have not been altered by scattering, and have preserved the same frequency as the original beam. A second hump is the evidence of the "modified" scattering; the relative amounts of the two vary with the scattering substance. Every material returns something of both kinds; but the lighter the atom the more intense is the modified in comparison with the unmodified; at the same time, as already said, the change of wave-length is constant from substance to substance. The phenomenon is a little like that of fluorescence, but the likeness is only on the surface. But this is true of both, that the undulatory theory does not suggest a simple explanation in either case. I ought to say that Professor Raman, of Calcutta, has just announced in "Nature" that he is able to explain the Compton effect on classical lines, and promises to give his new theory in full.

On the other hand, the corpuscular theory gives at once an explanation which is relatively simple, and allows quantitative deductions to be made which are in entire agreement with the facts. If a corpuscle or "photon," to use the term which has lately been suggested, comes into collision with an electron, as it can do in traversing any material, it is possible to work out the consequences as if for an impact between two billiard balls, assuming only that photon and electron have both of them momentum and energy, the quantities being reckoned according to rules already established. The result is that the photon goes off with somewhat diminished energy; it has lost "frequency," and the calculated loss is exactly that which is shown in Fig. 3 as the result of experiment. Moreover, the electron ought to start off with a certain amount of energy, and when Wilson's photographs are examined the predicted tracks are found on them. Thus the spectrometer, which is based on an undulatory theory of X-rays, has established facts which are in accord with the corpuscular theory.

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* "X-Rays and Electrons." By A. H. Compton. (New York: D. Van Nostrand Co., 1926.)

When we see in this way that radiation, which has always been thought of as undulatory, can on occasion display corpuscular properties, we are prompted to ask whether moving electrons — β-rays, cathode rays, and so on — which we have always considered as corpuscular, may not behave sometimes like waves? The answer appears to be in the affirmative. In America Davisson and Germor have recently described most remarkable experiments in which moving electrons are reflected by crystals in a manner bearing an obvious, though diffuse, resemblance to the now well-known reflection of X-rays, and the latter is always explained on the basis of an undulatory theory. More recently still, Prof. G. P. Thomson, of Aberdeen University, has shown that when electrons are shot through a very thin film of metal, haloes are formed upon a

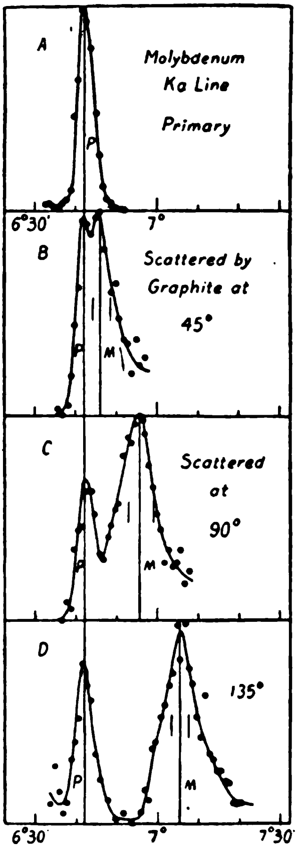

Fig. 3.

(*Courtesy of D. Van Nostrand Company, Inc.*)

Ascissæ represent angle of scattering and ordinates representing intensity. The hump on the left of each figure represents the intensity of the scattered radiation, which has the same wave-length as the primary beam. The hump on the right marked M represents the intensity of the modified portion of the scattered radiation.

This figure shows how the effect depends on the angle of scattering.

photographic plate on the other side of the metal, which may be explained qualitatively and quantitatively if the electrons act as waves in passing through the minute crystalline particles of the metal film. Prof. Thomson has promised, I am glad to say, to give us an account of this work in a Friday Evening Discourse after Easter, and this will follow naturally on the lectures by Dr. Schrödinger and Prof. Whittaker.

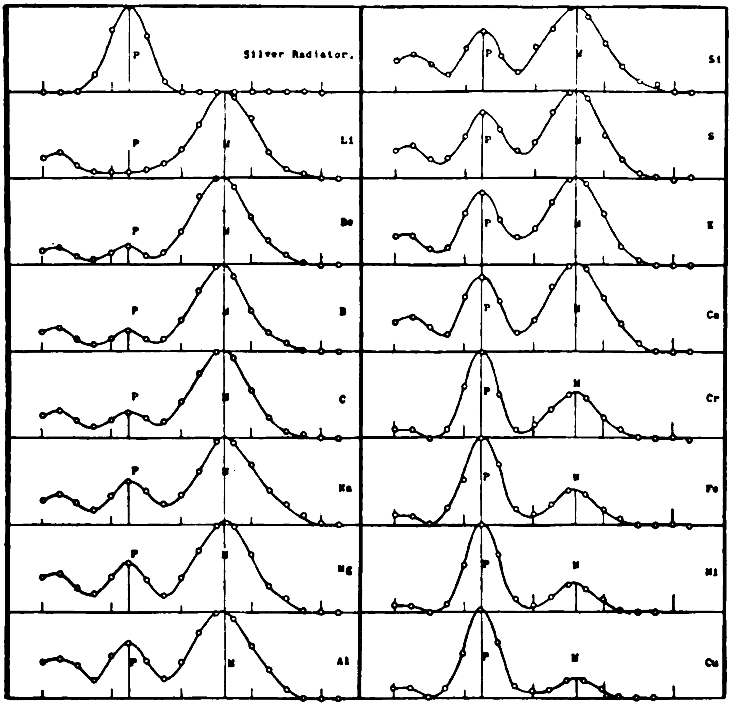

Fig. 4.

(Courtesy of D. Van Nostrand Company, Inc.)

In Fig. 2 the scattering substance is modified, and it is to be observed that the less the atomic weight of this substance the greater is the relative magnitude of the modified portion.

I hope that this brief sketch of the present position will give some ideas of its extraordinary and fascinating difficulties. Perhaps the details of the picture are sorting themselves out, and we shall see before long where we have failed to see some important point which was required for the complete resolution. When the picture is finally clear there will no doubt be atoms in it, electrons, wave motions, energies, momenta, and so on. But have we got them all rightly joined up? Perhaps wave motion belongs to more than the photon, or to something else than the photon? We can only wait.

[W. H. B.]

WEEKLY EVENING MEETING,

Friday, January 27, 1928.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

MISS D. A. E. GARROD, M.A. B.Sc.,
Author of "The Upper Palæolithic Age in Britain."

Prehistoric Cave Art.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, February 3, 1928.

Sir Almroth E. Wright, K.B.E. C.B. M.D. LL.D. F.R.S.,
Vice-President, in the Chair.

E. C. C. Baly, C.B.E. F.R.S.,
Professor of Inorganic Chemistry, University of Liverpool

Photosynthesis.

There is no process within the confines of Chemistry which is of greater interest and importance than that by means of which the living plant prepares the food on which its life and growth depend. This food consists of starch and sugars, together grouped under the general name of carbohydrates, and of certain nitrogen-containing compounds known as proteins. The material from which the plant starts is carbonic acid, or a solution of carbon dioxide, which it obtains from the air, in water which it obtains through its roots from the soil. From this substance alone the plant is able to prepare its supply of carbohydrates, and it is true to say that this chemical process is the fundamental basis of the whole of terrestrial life. This may truly be asserted because the production of the proteins is very closely associated with it and the initial stage is common to the two.

The formation of carbohydrates from carbonic acid when expressed by a chemical equation looks simple enough. There is no doubt that the first product of the process that can be recognised in the plant is a simple sugar, and thus the equation can be written

$$6H_2CO_3 = C_6H_{12}O_6 + 6O_2$$

where the simple carbohydrate is either glucose or fructose. These simple sugars undergo condensation immediately they are formed to give cane sugar or one of the starches, and these changes can readily be written as simple chemical equations.

The mechanism by means of which the plant achieves the synthesis of these complex compounds from carbonic acid has long been a mystery to chemists and to botanists. It is known that the agency used by the plant to effect its purpose is sunlight, and thus the term photosynthesis has been applied to the operation. It is also known that the plant makes use of certain pigments, such as chlorophyll, and it is to these that the colour of the leaves is due. The mystery of it all lay in the fact that no one knew what actually takes place, and, indeed, the more chemists and botanists explored, the more puzzling did the problem seem to be.

Perhaps the most puzzling fact of all is that the plant only makes use of sunlight, when all our previous knowledge of light reactions leads us to believe that such light is quite incapable of inducing photosynthesis. This may readily be understood if the amount of energy involved in the synthesis is considered. It has been proved experimentally that in order to synthesise one gram molecule (180 grams) of glucose or fructose there must be supplied to the carbonic acid a minimum quantity of energy equal to 673,800 calories. Whilst it is known that the plant manages in some way to absorb the necessary energy in the form of light, the physicist tells us that it cannot directly absorb enough energy from sunlight. Thus the photosynthesis can be brought about by red light of the wave-length $660\mu\mu$ when the energy directly absorbed can only be 260,000 calories, which is far below the minimum quantity required.

The experience gained from the ordinary reactions of photochemistry leads to the belief that if it is required to convert carbonic acid into sugars by means of light alone, it will be necessary to use ultra-violet light which is absorbed by carbonic acid, that is to say, light of wave-length $210\mu\mu$. It is obvious from this that some unknown factor is operating in vital photosynthesis.

In any endeavour to elucidate the mystery, it is evident that the first line of enquiry must be to study the action of the short wave ultra-violet light upon carbonic acid. This was first investigated by Moore and Webster in 1913, who found no evidence of any reaction taking place. They found, however, that in the presence of certain catalysts, such as colloidal iron hydroxide, small quantities of formaldehyde were produced. Since these results appeared to be at variance with general experience in photochemical investigations, they were again examined some years later in Liverpool, and it was then found that when a stream of carbon dioxide was passed through water irradiated by the light from a quartz mercury lamp, small quantities of formaldehyde were produced. This result seemed to be very satisfactory, since the formaldehyde could be looked upon as an intermediate stage on the way to carbohydrates, especially in view of the fact that Moore and Webster had proved that formaldehyde was converted by light into substance, with properties similar to the simple sugars.

Our observations were criticised by Porter and Ramsperger, who stated that if rigid precautions were taken to guard against the presence of every trace of impurity, no formaldehyde was produced. The suggestion was implied by them that the origin of the formaldehyde was to be found in some unknown impurity. There is, however, an alternative possibility, and one which is more in keeping with the known facts of the natural photosynthesis in the living leaf. There is no doubt that in this reaction the carbonic acid is converted directly into carbohydrates and that formaldehyde as such is not produced, and it seemed that the most probable explanation of the discrepancy between our results and those of Porter and Ramsperger was that the action of the ultra-violet light is to establish a photochemical equilibrium.

$$6H_2CO_3 \rightleftarrows C_6H_{12}O_6 + 6O_2,$$

which reverts to carbonic acid again in the dark. In the presence of oxidisable impurities a small amount of carbohydrates will be formed, which will be photochemically decomposed to formaldehyde. This decomposition of all the carbohydrates by means of ultra-violet light is well known.

There is no need to give here the details of the experiments which were carried out to test this view, and it is sufficient to say that conclusive proof was obtained of the reality of the equilibrium: that is to say, carbohydrates were found to be present in the solution during irradiation by ultra-violet light, and these vanished very quickly after the irradiation was stopped.

This gave us at once a starting point, because it seems evident that if a harmless inorganic reducing agent were added to the solution, carbohydrates should be formed in quantity on exposure to the ultra-violet light. Such a reducing agent is ferrous bicarbonate, and great hopes were raised when it was found that a saturated solution of this compound, which was completely colourless when prepared, gave a copious precipitate of ferric oxide on exposure to ultra-violet light. It was evident that the oxidation took place by reason of the oxygen in the carbohydrate equilibrium in accordance with the equation

$$4Fe\,(HCO_3)_2 + O_2 = 2Fe_2O_3 + 4H_2O + 8CO_2$$

and indeed it was found that on evaporation of the exposed solution a simple sugar was obtained. The quantity produced was very disappointing and far less than was anticipated, and the conclusion was forced upon us that some unknown factor was taking part in the process.

During many unsuccessful endeavours to improve the yield of the carbohydrates, it was noticed that the ferric oxide was not produced in the body of the solution, but only on the walls of the quartz containing vessels and on the surface of the iron rods used to make the bicarbonate. This led us to suspect that the surface was a determining factor, and we at once changed the experimental method so as to increase the surface as much as possible. In order to secure this a suspension of pure aluminium powder in water, maintained by a stream of carbon dioxide, was exposed to ultra-violet light. Increased yields of carbohydrates were at once obtained, but it was also found that the best yields were obtained when the aluminium powder had been allowed to coat itself with hydroxide by remaining in contact with the water before the exposure to light. This latter observation very materially changed our ideas, since it established the fact that the surface phenomenon is of far greater importance than the reducing action, and indeed raised the question as to whether the latter plays any rôle at all in the process.

In order finally to decide this question an aqueous suspension of pure and freshly prepared aluminium hydroxide, maintained by a stream of carbon dioxide, was exposed to ultra-violet light. There was obtained after filtration and evaporation of the solution a quantity of carbohydrates equal in weight to that produced when aluminium powder was used. This conclusively proved the fundamental significance of the rôle played by the surface, and at the same time the reducing action was found to be entirely unnecessary. Identical results were obtained with other powders, such as aluminium, zinc, and magnesium carbonates.

During the course of these experiments it occurred to one of my students (Dr. W. E. Stephen) that if a green powder were used in place of the white ones the photosynthesis might take place in visible light, the green colour being suggested by the green colour of the plant-pigment chlorophyll. This was found actually to be the case, since a suspension of nickel carbonate maintained by a stream of carbon dioxide in water, on exposure to the light from an ordinary tungsten filament lamp, gave a larger yield of carbohydrates than any of the white powders in ultra-violet light. We soon found that there was no especial virtue in the green colour, and that equally good results were given by pink cobalt carbonate.

Apart from the interest which accrues from the fact that the photosynthesis is thus achieved in a way which shows a real analogy with the natural phenomenon, the method with a coloured surface and visible light has the very material advantage in that the danger of photo-chemical decomposition by ultra-violet light is completely eliminated, with the result that the products are obtained in a purer state.

From the above description of the direct photosynthesis of carbohydrates from carbonic acid in the laboratory several points arise which require discussion and explanation. In the first place it may be stated that the most rigid control experiments which we could devise have definitely established the fact that the carbohydrates are not due to the presence of impurities.

One of the greatest difficulties met with in this work was the preparation of the various materials used for the surfaces, since it is absolutely essential that these be completely free from any trace of alkali. It is well known that when metallic hydroxides and carbonates are precipitated they tend to absorb the alkali, and the removal of this is extraordinarily troublesome. The absence of any alkaline reaction in the filtrate after the powder has been boiled with water is no criterion of purity, and the only satisfactory method is to pass carbon dioxide into a suspension of the powder in water for two hours in the dark, and the filtrate after concentration must yield no weighable quantity of alkaline carbonate.

It was frequently found that the carbonates of nickel and cobalt, even when completely freed from alkali, were entirely ineffective in promoting photosynthesis. These can, however, be activated either by heating to 120° or by exposure in thin layers to ultra-violet light, and this fact afforded a very convincing method of carrying out controls. A quantity of one of these inactive powders gives no trace of carbohydrates when exposed to visible light in the manner described. The same sample of powder when activated and used in the same apparatus, with the same water, the same light, and carbon dioxide from the same source, gives a good yield of carbohydrates. So, once and for all, is all doubt removed as to the possible effect of impurities.

For the benefit of those who may wish to repeat these experiments, it may be stated that more recently it has been found possible to prepare nickel carbonate by a new method which is free from the objections characteristic of its precipitation by means of alkali carbonate. A solution of carbonic acid in conductivity water is electrolysed, the electrodes being made of nickel plates. The current is taken from a 220 volt circuit, and sufficient resistance is intercalated to reduce the current density to from 1 or 2 ampères per sq. dcm. The electrolyte is cooled by glass coils through which a stream of water is maintained. With electrodes 190 sq. cm. in area it is possible to prepare 30 grms. of pure carbonate in 24 hours. The carbonate should be collected every day by filtration, and it is advisable to clean the electrodes with emery paper every third day.

To sum up the results, so far as they have been described, it has been found possible in the laboratory to produce carbohydrates directly from carbonic acid by a process which is physically similar to that of the living plant. The essential difficulty in our understanding of the natural photosynthesis has been solved, namely the use of visible light as the agent in a process which the elementary laws of photochemistry taught us to believe could only be achieved by means of ultra-violet light. As so often happens the explanation when found is very simple. The great amount of energy required to convert the carbonic acid into carbohydrates is supplied to it in two portions, one by the surface and the other by the visible light.

Nothing has as yet been said of the actual carbohydrates which have been photosynthesised in the laboratory. Although as yet our information is still meagre, there is no doubt that the photosynthetic syrup is a mixture containing glucose or fructose, or both. There are also present more complex carbohydrates, which can be resolved to the simple sugars by the action of dilute acid. The analogy with the products of natural photosynthesis is too close to be passed by without comment.

Although it has not as yet been possible to carry out a complete analysis of this syrup, owing to the difficulty of preparing a sufficiently large amount, interesting information has been gained from the investigation of the sugar syrup obtained by the action of light upon formaldehyde solution. This has been pursued during the last three years. We owe a debt of gratitude to Sir James Irvine for the signal help he has given us in this work. He himself was the first, in association with Dr. Francis, to prove that glucose is one of the substances actually produced. By oxidation of the sugars to the acids by means of bromine, and the crystallisation of the salts of these with brucine, cinchonine and quinine, we have obtained d-gluconic and also d-erythronic acids. This not only confirms Irvine and Francis in their proof of glucose, but it also proves that fructose is formed just as in the living plant. In addition to that, there is produced a mixture of complex acids which afford convincing evidence of the synthesis of complex carbohydrates.

Although it may be thought that the use of formaldehyde as the starting point takes away something from the interest, yet it must be remembered that it makes but little difference whether in actual fact we start from carbonic acid or formaldehyde. Without doubt the first substance, transiently formed in either case, is the same, namely activated formaldehyde which polymerises to the sugars.

The similarity between the vital and the laboratory processes is not confined to the fact that the products from the two are the same. Botanists tell us that in the living plant the photosynthesis takes place on a surfaçe, so also is a surface necessary in the laboratory. It has been found possible to compare the quantities of carbohydrates synthesised for equal areas exposed to light in the case of living leaves and the glass vessels of the laboratory. These quantities are about the same. Some plants produce more and others produce less than we are able to synthesise. This similarity may be emphasised, because surely Dame Nature in the living leaf has produced the best machine she could for her purpose of food production for her children of the vegetable kingdom.

There is yet another striking feature which is common to the two, photosynthesis *in viro* and *in vitro*. The light must not be too strong in either, for if it is too strong then harmful results at once supervene. This is due to the poisoning of the surface by the oxygen which is set free. In both cases this poisoning slowly rights itself, and in both the synthesis must not proceed at a greater rate than that of the recovery of the surface from its poisoning.

In fine, so far as we have been able to carry the investigations, the processes in the living plant and in the laboratory show most striking resemblance, not only in the compounds which are formed, but in every feature which is characteristic of either of them.

For my own part I would go further than this, because I believe that these experimental results help us to gain some understanding of the chemistry of life, the chemistry which is so different from that of man's achievements with his test tube, flask and beaker. Within the confines of vital chemistry reactions take place which are so far outside our own experimental experience that it came to be believed by many that they were under the control of a mysterious force, to which the name of *vis vitalis* was given. One of these processes has come within our purview to-night, the condensation of the simple sugars, glucose and fructose, to form cane sugar, starch and inulin. No one has yet succeeded in effecting these syntheses in his laboratory, but it would seem that something of that nature takes place in our photosynthesis. Why then is it that even this step forward has been gained?

The one lesson that we have gained from photosynthesis is, that the definitive factor is the very large amount of energy which must be supplied to the carbonic acid before the synthesis of the simple sugars takes place. The means of supplying that energy do not concern the argument. The synthesis proceeds at an energy level which is far higher than is the case in the reactions of ordinary chemistry, and the sugars are formed at that high energy level. I myself believe that the condensation reactions to give the more complex carbohydrates are those which are characteristic of the simple sugars when they exist at the high energy level. The reason why no one has succeeded up till now in inducing these reactions to take place is because no one has hitherto been able to supply the large energy increment necessary.

I myself believe that we find in this the key which unlocks the door of vital chemistry, and that the chemistry of all life is one of high energy, our laboratory experience being confined to the chemistry of low energy. From this view point I see a wondrous vista unfold itself, wherein new understanding, new hopes and new possibilities reveal themselves. Health and vitality must essentially depend on the high energy level being maintained; any lowering of that level will lead to poor health and weak vitality. Knowledge comes to us of the means whereby the high level may be kept unimpaired. The most important sources from which we can absorb high energy are fresh food and ultra-violet light. From the one we learn the necessity of the rapid distribution of our food supply before its high energy is lost, from the other we gain a real understanding of the benefits of ultra-violet light therapy, and, more important still, of the dangers of its misuse. We gain an insight into the chemistry of vitamins, which in the light of our new knowledge reveal themselves as stores of high energy, bottled sunshine so to speak, which yield their energy to restore and maintain the vitality of decadent tissues. A vision thus comes to us of a new chemistry with limits far flung beyond those which constrain our knowledge of to-day, a chemistry which will embrace and co-ordinate not only the properties of inanimate matter upon this earth, not only the wondrous mechanism of the life of man in health and in disease, but in addition the stupendous marvels of the birth and growth of the worlds outside our own. From those who would decry this as a mere speculation I beg forgiveness, and plead that speculation based on sure experimental fact is the life blood of true scientific research.

[E. C. C. B.]

GENERAL MONTHLY MEETING,

Monday, February 6, 1928,

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Edgar Howard Collins,
The Rt. Hon. J. C. C. Davidson, P.C. C.H. C.B. M.P.
J. J. Fox, D.Sc. O.B.E.
Miss F. E. Gollan,
Miss Charline Wheelock Hervey,
Charles Hogg, M.A.
Earl of Iveagh, C.B. C.M.G.
John Lea, M.A.
Miss Thora Cuthbert Marwick,
Walter Rosenhain, D.Sc. F.R.S.
Mrs. Stella Sanderson,
Lord Swinfen,

were elected Members.

The Special Thanks of the Members were returned to Mr. Robert Mond, M.R.I., for his valuable gift of the major portion of the MS. of Faraday's "Chemical Manipulation"; to Mr. J. G. Gordon, M.R.I., for his generous Donation of £200 to the General Fund; and to Professor E. C. C. Baly, for his present of the first Sugar synthesised from Carbonic Acid.

The Chairman reported that the Managers had received a Bust of the late Mgr. Vay de Vaya, bequeathed by him to the Royal Institution "in memoriam" of his Friday Evening Discourse in 1904 on Corea.

The Chairman reported the death of Professor H. R. von Groth on December 1, 1927, and of Professor H. A. Lorentz on February 4, 1928, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Institution has sustained by the death of Geheimrat Professor Dr. Paul Heinrich Ritter von Groth, Ph.D. Hon. D.Sc.(Cambridge); Professor of Mineralogy and Geology in the University of Münich; Foreign Member of the Geological Society; Foreign Member of the Royal Society; and Honorary Member of the Royal Institution.

Professor von Groth was one of the oldest workers in the field of Crystallography, and discovered, in 1870, that the effect of the replacement of Hydrogen by other atoms or groups in a crystal was to cause a change in its crystalline form—a discovery which was instrumental in arousing considerable activity in researches on Atomic Structure. He was the founder and first editor of the Zeitschrift für Kristallographie (1877), the author of important works on Mineralogy and Crystallography, and of numerous contributions to the memoirs and journals of various Scientific Academies.

The Managers desire to express, on behalf of the Members, their sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Institution has sustained by the death of Professor Hendrik Antoon Lorentz, Ph.D. Hon. Sc.D.(Cambridge); Honorary Professor of Mathematical Physics, University of Leyden; Director of Research at the Teyler Institute, Haarlem; Nobel Laureate (Physics), 1902; Copley and Rumford Medallist and Foreign Member of the Royal Society; Honorary Member of the Royal Institution.

The earliest researches of Professor Lorentz were devoted to the study of the Reflection and Refraction of Light; he subsequently made many investigations on the Theory of Electrons and the Electro-dynamical Constitution of Matter. He was the author of the Theory of Electrons (1916), Lectures on Theoretical Physics (1919), and many other works, and also contributed numerous memoirs to the Proceedings of the Amsterdam Academy and other societies.

He delivered a Friday Evening Discourse at the Royal Institution on June 1, 1923, on the Radiation of Light.

The Managers desire to express, on behalf of the Members, their sincere sympathy with the family in their bereavement.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Brett-James, Major Norman G., F.S.A. (the Author). The History of Mill Hill School (1807-1923). 8vo. 1927.

British Cotton Industry Research Association, The Director—Research in the Cotton Industry. 8vo. 1927.

Salvage, Lieut.-Col. J. V., M.D. M.R.I.—Biographical History of Guy's Hospital. By S. Wilks and G. T. Bettany. 8vo. 1892.

The Authors of England: A Series of Portraits. Engraved by A. Collas. Illustrated Notices by H. F. Chorley. 4to. 1838.

Tokyo, Imperial University—Scientific Papers of S. Sano. Edited by K. Térazawa. 8vo. 1926.

The following Publications of Scientific Societies and Periodicals have been presented or received in exchange during 1927 :—

Lords of the Admiralty—Greenwich Observations, 1925. 4to. 1927.
Report of Astronomer Royal to the Board of Visitors, 1927. 4to.
Determination of Wave-Lengths of Stars, 1902-25. 4to. 1927.
Cape Astrographic Zones, Vol. IX. 4to. 1926.
Report of H.M.'s Astronomer at the Cape of Good Hope, 1926. 4to. 1927.
Nautical Almanac, 1929. 8vo. 1927.
British Museum Trustees—Handbook to Ethnographical Collections, 2nd Edition. 8vo. 1925.
Stone Age Antiquities, 3rd Edition. 8vo. 1926.
Schools of Illumination, Part 5. 4to. 1926.
Catalogue of Additional MSS., 1911-15. 8vo. 1925.
Index Animalium, Sectio Secunda, Parts 10-12. 8vo. 1926-27.
Flora of Jamaica, Vol. V. 8vo. 1926.
Guide to the Crustacea. 8vo. 1927.
Secretary of State for India.
Geological Survey:—
Palæontologia Indica: New Series, Vol. VII. No. 3; Vol. IX. No. 2; Vol X. Nos. 1-2; Vol. XIV. 4to. 1927.
Records, Vol. LIX. Parts 1 and 4; Vol. LX. Part 1. 8vo. 1927.
Kodaikanal Observatory Report, 1926. 8vo.
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Report on Madras Government Museum. 1927. 4to.
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Agricultural Society, Royal—Journal, Vol. LXXXVII. 8vo. 1926.
Allegheny Observatory—Publications, Vol. VI. Nos. 8-9; Vol. VII. No. 1. 4to. 1927.
American Academy of Arts and Sciences—Memoirs, Vol. XV. No. 4. 4to. 1926.
Proceedings, Vol. LXI. No. 12; Vol. LXII. Nos. 1-4. 8vo. 1926-27.
American Geographical Society—Geographical Review, 1927. 8vo.
American Philosophical Society—Proceedings, Vol. LXV. Nos. 3-5. 8vo. 1926.
Amsterdam, Royal Academy of Sciences—Proceedings, Vol. XXVIII. 8vo. 1925.
Verslag, Band XXXIV. 8vo. 1925.
Verhandelingen, 2e Sectie, Deel XXIV. Nos. 3-4; Deel XXV. No. 1. 8vo. 1925-26.
Jaarboek, 1925-26. 8vo.
Antiquaries, Royal Society of—The Antiquaries' Journal, Vol. VII. 8vo. 1927.
Archæologia, Vol. LXXVI. 4to. 1927.
Argentina, Association of Electrical Engineers—A. Volta, 1745-1827. 8vo. 1927.
Asiatic Society of Bengal—Journal and Proceedings, Vol. XXI. Nos. 4-6; Vol. XXII. Nos. 1-4. 8vo. 1927.
Asiatic Society, Royal (Bombay Branch)—Journal, N.S., Vol. II. No. 2. 8vo. 1927.
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Geophysical Supplement, Vol. I. Nos. 8-9. 8vo. 1926.
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Birmingham Philosophical Society—Proceedings, Vol. XV. Part 6. 8vo. 1927.
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WEEKLY EVENING MEETING,

Friday, February 10, 1928.

JOHN MITCHELL BRUCE, C.V.O. M.D. LL.D. F.R.C.P.,
Manager and Vice-President, in the Chair.

B. MELVILL JONES, A.F.C., Francis Mond Professor of
Aeronautical Engineering University of Cambridge.

Research on the Control of Aeroplanes.

AN essential factor in the control of anything, whether it be a motor car, a ship, or an aeroplane, is some means of applying the necessary force in the desired direction. The car requires its steering wheel, the ship its rudder. But this is not the only factor which decides the character of the control, whether quick or slow, difficult or easy. Other factors inherent in the craft to be controlled have to be considered. One such factor is inertia. A motor car has very little inertia compared with the forces which can be exerted through the road wheels; consequently it responds at once to a movement of the steering wheel and the turning stops immediately the wheel is centralised. A ship has great inertia compared with the force exerted by the rudder; consequently the rudder must be applied some time before an appreciable turn is started, and reversed *against* the movement long before the ship has swung to its new course. The character of the control of a ship is thus entirely different from that of a car on account of this factor of inertia, which has nothing to do with the controls themselves.

Another factor is the stability or instability of the motion when the controls are not moved. A stable motion is one which, if slightly disturbed, will settle back into its original form; an unstable motion is one which, after a slight disturbance, will depart further and further from the original form. In a modern car the pivots about which the wheels turn in steering are arranged like the casters of a chair, so that the wheels have a slight tendency to turn to the side towards which the car is trying to slip. If the car moves round a curve the centrifugal force makes it try to slip outwards, and the wheels, left to themselves, turn outwards and straighten the path. Such a car is stable and easy to drive straight; if the wheels had the opposite tendency it would be unstable, and the driver would have continually to be correcting tendencies to swerve to one side or the other. An unstable car is not impossible to control and may even, by practice, come to be controlled by unconscious reflex action, with no more fatigue than is felt in walking, but experience has shown that it is better to make it stable. It should not, however, be too stable, for then it will be heavy to steer round corners at high speeds, when the centrifugal force is large, and it will try to run down hill, so that, when travelling on the side of the camber of the road, it will have continually to be held out of the ditch.

The motor car thus illustrates some important points which are common to the control of any kind of craft: although the control of an unstable craft may not be impossible, stability is on the whole desirable, but too great stability may introduce other undesirable qualities and may be as bad or worse than instability. A condition which might be described as benevolent neutrality is generally sought.

Again a craft may be stable, in the sense that it tends to return when disturbed from a straight path, but it may overshoot on the other side further than the original deviation, so that an oscillation of increasing magnitude may arise. Such an increasing oscillation is said to contain a negative damping term. Conversely an oscillation which tends to decrease is said to contain a positive damping term. An example of a negatively damped oscillation occurs when a yacht's dinghy, loaded by the bows, is towed by a short rope. As the tow rope is shortened the dinghy starts to yaw from side to side with increasing violence and may ultimately be swamped.

A negatively damped oscillation is difficult to control; an inexpert hand on the controls generally makes it worse. It is even possible to convert a truly stable motion into an increasing oscillation by inexpert use of the controls; this is generally due to lag, or the time interval between the impulse to control and the muscular response. The skidding car, moving from side to side with increasing violence in the hands of the inexpert driver, affords a good example of this type of motion.

I hope I have succeeded in impressing on you, by appeals to familiar experience, that the study of control involves more than the control organs themselves. I have illustrated this theme by types of motion which will reappear later in the lecture.

Long before successful man-carrying aeroplanes were developed a cleavage of opinion on matters of control was apparent. One side, which contained on the whole the practical men who tried to fly themselves, was mainly concerned with providing control organs and acquiring the skill to use them. The other side, which contained mainly the theorists and the constructors of non-controlled models, were mainly concerned with the stability of the uncontrolled craft. This division of opinion continued well into the war and traces of it are still present. I cannot resist the temptation to attempt a short historical sketch of this controversy, for I think that by so doing the present situation will become more clear and I can use the sketch to illustrate certain important points in the control problem.

The great problem before the protagonists of the control school was to remain alive long enough to achieve sufficient practice and perfect their apparatus. Their difficulty was greater than they could guess. Nature laid a trap for them, the full cunning of which we are only just beginning to realise. To understand this trap we must look a little into the matter. The first essential of steady flight is that the air shall exert a lift on the wings equal to the weight of the aeroplane. This lift depends upon speed through the air and upon the incidence, or angle at which the wings strike the air. A typical relationship is shown in Fig. 1. If, in this example, the aeroplane weighs 2,000 lbs., it cannot be supported at speeds under 50 m.p.h. At high speeds support can be obtained at a small incidence, but if the speed drops slowly towards 50 m.p.h., a point is reached at which the incidence will have to increase rapidly, and just above 50 m.p.h. flight will be possible at two alternative incidences, one considerably larger than the other. The minimum possible speed is called the stalling speed; the critical incidence, at which the minimum speed is just possible, is called the stalling incidence, and an aeroplane flying above this incidence is said to be stalled. So long as the speed is considerably greater than the stalling speed the achievement of stability and control is relatively easy, but as the speed falls and the stalling angle is approached changes occur in the air-flow about the wings which, unless special precautions are taken, render the aeroplane violently unstable and simultaneously destroy the power of control.

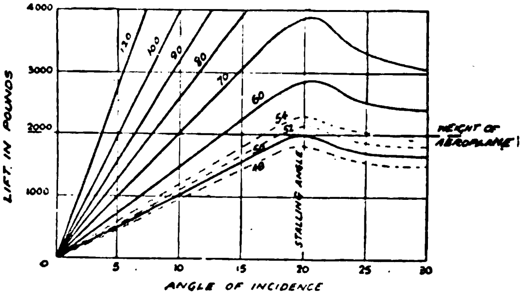

FIG. 1.—The Aerodynamic lift, at different speeds and wing incidences, of an Aeroplane which stalls at 50 m.p.h. (Numbers on curves show speed in m.p.h.)

Now you perceive the nature of the trap. The early pioneers perhaps succeeded, with the help of the wind, in taking off at a speed greater than the stalling speed, which for their machines was very low. Possibly they carried out many flights without stalling and, delighted with the ease of control, were emboldened, on some favourable day, to glide to a considerable height above the ground. Sooner or later they were bound to stall and, if high up at the time, to kill themselves; for the motions following a stall are peculiarly violent and liable to lead to heavy impact with the ground.

Herein lay the wisdom of the Wrights, the greatest exponents of the control school of thought. Warned by the experience of others they suspected some such trap and never in all their early work allowed themselves to get more than a few feet from the ground. They must have experienced the stall or the approach to the stall, for they discovered what is now known to be the simplest, though not the only counter to it—a powerful rudder. That was the turning point in Aeronautics, when the Wrights managed to get trained in control without being trapped and killed by the stall. After this, increased engine power and experience enabled the stall to be avoided more easily and deaths from this cause became relatively less frequent, though it has still remained the principal cause of fatal accidents up to the present time. Early progress, however, lay all in the improvement of normal flight, and the study of the stalled condition was not taken up seriously until after the War. We also shall leave it till later in this lecture.

The Wrights achieved their success with an aeroplane which was definitely unstable in several ways, counteracting the effects of this instability by acquired skill. Early design naturally followed this lead, and stability came to be regarded by many of the pioneer flyers as of no practical interest and indeed as a kind of bogey, invented by scientists for their own glorification. After a few important, but not fundamental, changes from the form in which the Wrights created them, controlled organs crystallized by about 1911 into a form typified by the BE 2A Aeroplane, produced in the government factory at Farnborough, under the guidance of Geoffrey de Havilland. Since that time the method of control, except for the introduction of balancing devices to lighten the pilot's effort on large aeroplanes, has scarcely altered, and the interest in the story shifts to the side of stability.

In 1896 Langley had worked out the general principles of stability sufficiently to make a model, driven by a small steam engine, which was so stable and well balanced that it flew a distance of over three-quarters of a mile.

About the same time Lanchester, working with smaller gliding models, succeeded in unravelling the complicated factors which influence the stability of an aeroplane in normal flight (below the stalling angle), and his results, which he collected in a volume published in 1908, contained in essence most of the principles of practical importance which we employ to-day in the calculation of stability.

About 1911 two interesting things happened. Captain Dunne made and flew successfully his remarkable tailless aeroplane with swept-back wings, which was undoubtedly extremely stable, probably far too stable for comfortable flying; and Bryan published a book in which he showed how calculations upon the stability of aeroplanes could be brought into line with conventional mathematics. Dunne's line of development was not followed up, but we shall see something of the sort cropping up again towards the end of the lecture. Bryan's book was the foundation of modern methods of calculating stability.

Bryan's idea was to measure the effects of simple disturbances, such as a rolls or a pitches separately, and thus to obtain a number of characteristic quantities, or "derivatives" as they are called, which could be used in the calculations of stability. He did not have the means to make the necessary measurements, but the scientists of the National Physical Laboratory, who were already in possession of wind tunnels, set to work to determine these quantities for small models and to make the necessary calculations along the lines which he had indicated. The experimental campaign so started has been in progress ever since.

The application of this systematic study of stability to man-carrying flying machines was first made in the Government Factory at Farnborough by Busk. He modified the unstable BE 2A into the stable BE 2C by relatively small changes in the position of the centre of gravity and in the area and arrangement of the fixed surfaces on the end of the tail. This was a distinct step forward, and much notice was taken of it in the Press, where the opinion was freely expressed, mainly by non-flyers, that the safety of flying was now assured. This view was wrong, the provision of stability alone is not sufficient for safety, as Busk and his fellow workers were well aware. Safety is mainly concerned with the taking off and landing of aeroplanes, when a rapid response to control is even more important than stability.

The opposing school of thought, lineal descendants of the pioneer flyers, were naturally aggravated by this widely advertised and erroneous view of the relation between safety and stability, and the breach between the two schools widened still further, culminating in two extreme examples, the very stable SE 5, produced in the government factory at Farnborough and the very unstable Camel designed by Sopwith. These two machines, diametrically opposed in every feature relating to stability, shared between them the brunt of the single-seater fighting during the later stages of the war; opinion ran high concerning their relative merits and the lines of thought which they represented. Readers of that remarkable diary 'War Birds' will find the view of the supporters of the SE 5 forcibly expressed, but I have heard the other side equally strongly maintained. These two machines marked the culminating point in the controversy to which I have referred. Later development has been all towards compromise slightly on the stable side of neutrality. Benevolent neutrality, as I have called it.

Though the experimental technique and the mathematical calculations necessary for a thorough study of control and stability are difficult and elaborate, the main results are simply and easily stated.

The pitching motions of an aeroplane depend upon what is called its weathercock stability. An aeroplane hung up in a wind so that it can rotate about a horizontal axis through its centre of gravity has weathercock stability if, like a weathercock, it desires to face the wind and returns to the same attitude on being disturbed. As with a weathercock, this kind of stability is increased either by moving the pivot—in this case the centre of gravity—forward, or by increasing the area of the tail.

A free flying aeroplane which has this form of stability will, if disturbed from steady flight, first rotate rapidly so as to restore the incidence to the equilibrium value and then execute a series of long slow pitching movements similar to a ship travelling over ocean waves. The length of these waves from crest to crest is between ½ and 1 mile, and the time taken some 25 seconds. These movements are so slow that they have little influence on control.

The weathercock *unstable* aeroplane, if pivoted like a weathercock, would very quickly turn round and face backwards. If this happened in free flight it would be unflyable, except by a pilot with the skill of a juggler, but it does not happen. Suppose the aeroplane is flying freely and the nose is accidently deflected upwards; being unstable it will throw up its nose still further, but at the same time the increased incidence will cause it to leap upwards with great suddenness. The direction of motion is thus *rotated upwards* faster than the aeroplane itself, so that the first quick adjustment is a fall of the incidence to nearly its original value. Subsequently the upward tilt and upward trend of the path increases relatively slowly, until the aeroplane slows up and stalls. There is, however, ample time for the pilot to correct this subsequent movement, provided that his attention has not wandered. This is the reason why the Wrights and others were able to develop flight on aeroplanes which were unstable fore and aft. So long as the instability is not too great, therefore, the aeroplane, like the motor car, can be controlled effectively whether it is stable or unstable.

As in the motor car, too great stability is definitely objectionable, partly because heavy forces are then required to execute rapid manœuvres and partly because heavy stick forces, or adjustments of some sort, are required to "trim" the aeroplane for different speeds. For alterations in speed must be accompanied by changes in incidence, which in a very stable machine will require large control forces. A neutral aeroplane, on the other hand, if trimmed for one speed will be in trim for other speeds within a wide range.

The problem here before the designer is so to adjust the centre of gravity and the tail areas as to produce a very slightly stable aeroplane.

The rolling and yawing motions of an aeroplane can also be simply described, though in detail they are very complicated.

When the aeroplane rolls the falling wing meets the air at a larger incidence than the rising wing and experiences a greater lift; a very large couple opposing the roll is thus generated. This is the predominating factor in the control, for it prevents rapid rolling and gives the pilot time to observe what is happening and correct it. This is the reason why the Wrights could control an aeroplane which was laterally unstable.

If the aeroplane has a "dihedral angle"—tips of the wings higher than the middles—side slip tends to raise the wing towards which the slip is occurring. If it has a large vertical fin on its tail it will turn or yaw towards the slip. In normal flight the dihedral angle has a powerful stabilising influence, because if one wing falls below the other, side slip will occur towards that wing, and a couple will be generated raising the wing.

An aeroplane which has too large a vertical fin on its tail and too little dihedral angle will have what is called spiral instability; if slightly disturbed from straight flight it will continue to roll and turn from its course, and ultimately descend in a spiral curve. This motion is, however, so slow in developing that it is of no importance so long as the pilot is in control, but like other forms of instability it is undesirable, particularly when long flights are contemplated.

Another motion possible to an uncontrolled aeroplane consists of a complicated rolling and yawing oscillation, generally of about six seconds period. This will become unstable and render the aeroplane practically unflyable if the vertical fin in the rear is too much reduced. Thus the exact proportions of these fin surfaces is a matter of great importance; if they are too small the relatively quick oscillations will become unstable, with disastrous results; if they are too large the slow spiral instability, which on the whole is undesirable, will occur. The problem is eased by giving a good dihedral angle, for this widens the limits permissible in fin size, without incurring either of these defects.

One other fact of interest has only lately come to light. If the dihedral angle is too small, the short period oscillation to which I have referred may become of the type which is caused to increase by a control which contains a lag. As there is always some lag between the pilot's intention and his performance, this latent defect may cause trouble; even though it may not be so bad as actually to cause the oscillations to increase, it may lead to great difficulty in damping them down. In bumpy weather, therefore, the oscillations may be continuous from one air bump to the next, with disastrous results both on the strength of the pilot and the stomachs of the passengers. The improvement in the bad weather qualities of some of the later cross channel aeroplanes is attributed partially to increased dihedral.

This concludes what I have to say about control in normal flight. I hope I have succeeded in explaining why the provision of good control qualities is more a question of proportioning the aeroplane and adjusting its load properly than of devising new control organs. These have changed little, as regards their general features, since before the War.

This brings us to about the end of the War. More data has accumulated and measurements have been refined, but most of the statements which I have made might have been made then. The trap which killed the early pioneers still, however, continued to take its toll of life, though in a much lower proportion to the hours flown. Ample power had become available to allow the normal flying speed to be so much greater than the stalling speed that accidental stalling became rare, except when a pilot was deliberately flying slowly with the object of making a landing. When it did occur, however, the same consequences followed as with the early pioneers, but the danger was increased, because of the higher loading and consequent heavier impact of the later machines. Throughout the history of flying to the present day this has been, and still remains, the most frequent cause of fatal accidents.

Shortly after the war the British Research Committee for Aeronautics started a research campaign into the causes and cures of the trouble. The research was carried on in wind tunnels, by theoretical work, and in actual flight at heights sufficient to rob the stall of its danger, with the result that the principles underlying the matter are now understood and several ways of eliminating the danger are known.

One small part of this campaign of research is being carried on by the University Air Squadron at Cambridge. Our task is to endeavour to obtain precise experimental records of the motions of stalled aeroplanes, both when left to themselves and when the pilot is trying to control them. The apparatus which we used was developed and constructed at the Government Research Establishment at Farnborough and lent to us for the purpose. It consists of a box containing three gyroscopes which are slightly deflected against a spring control when the aeroplane is turning, The deflection of each gyroscope is proportional to the rate at which the aeroplane is turning about some particular axis, and they are arranged so that, between them, they measure the three rates of turn about three axes mutually at right angles. These three records are recorded continuously upon a moving photographic film. Three other instruments record independently the movements of the three controls—elevator, rudder and aileron—and all these records are synchronised from a central clock which records half second intervals on all the films.

Figs. 2–9 show some results selected from over a hundred sets which we have obtained. These results, which I shall now describe, have all been predicted, at least in their general features, by calculations based on wind tunnel observations of the forces acting upon models, supported in various ways in the wind tunnels of the National Physical Laboratory and the Royal Aircraft Establishment: The wind tunnel experiments and these calculations were made long before precise records in free flight had been obtained, but for lecture purposes I shall reverse the chronological order of the events and describe the results first before explaining why they occur.

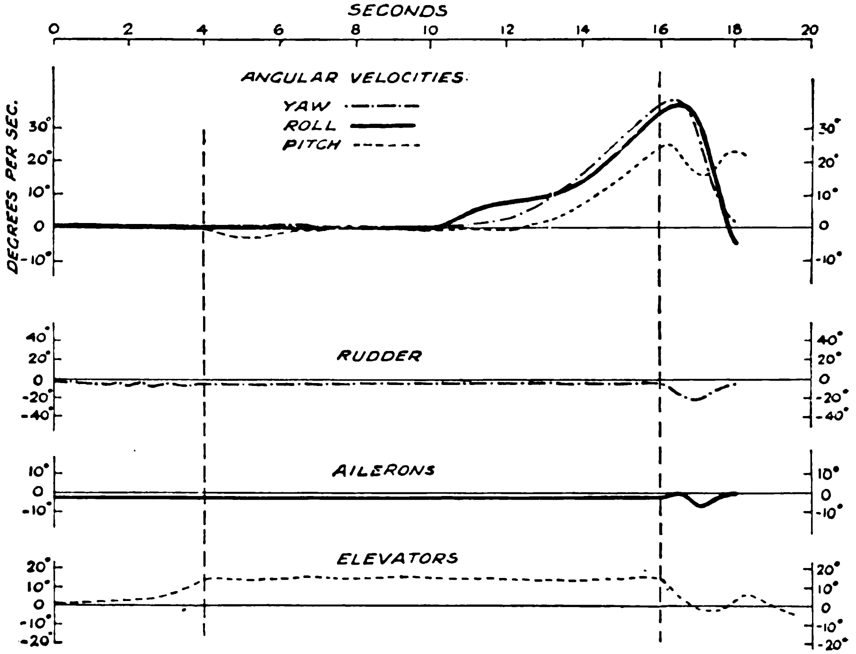

FIG. 2.—Controls fixed. The unstable rolling turn.

Fig. 2 shows a record of one such flight. The experiment began at the vertical line marked four seconds, when the elevator was pulled right back and the incidence of the wings (not shown in the figure) was between 19° and 20°. From that time the controls were held fixed until sixteen seconds, when the experiment ended. This experiment began exceptionally favourably, with no rotation of any importance occurring. Straight flight continued undisturbed until ten seconds, when some slight disturbance started the unstable motion characteristic of this aeroplane at this incidence. Increasing rates of rolling and yawing, both to starboard, were then recorded,

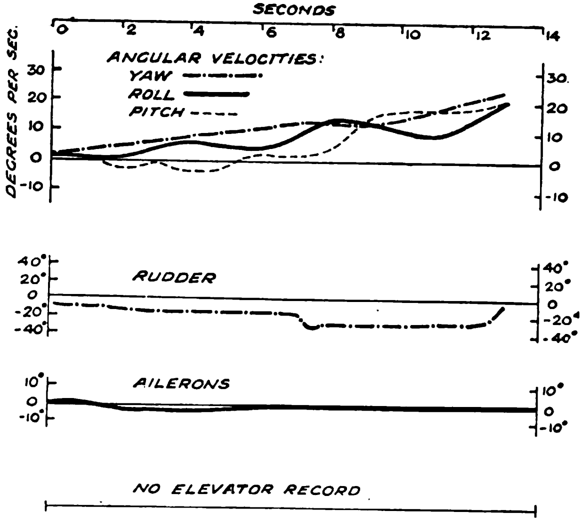

FIG. 3.—Controls fixed. The increasing oscillation.

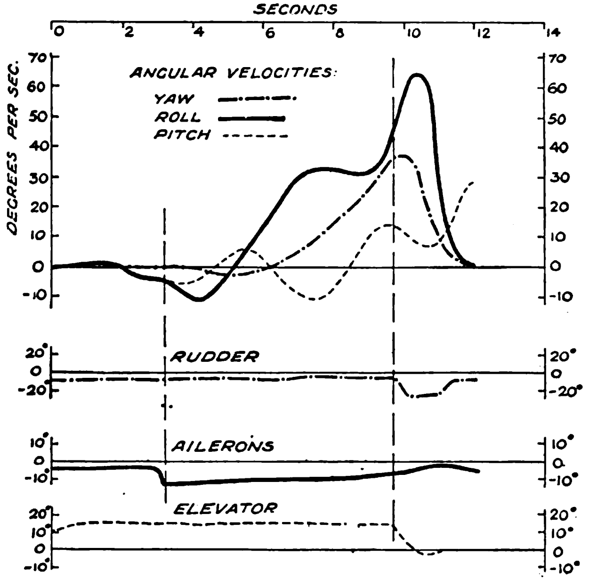

FIG. 4.—Failure of Ailerons applied during steady flight. (Note apparent success at first.)

which in six seconds had grown to some 30° per second. The aeroplane by this time had rolled through some 60° from the horizontal and the pilot then stopped the motion by pushing forward his elevator and reducing the incidence of the wings below the stalling point. The rapid check to the roll which followed this last control movement should be noted.

Superimposed on this unstable motion is a slight rolling oscillation ; this feature of the movement is by some accident more clearly shown

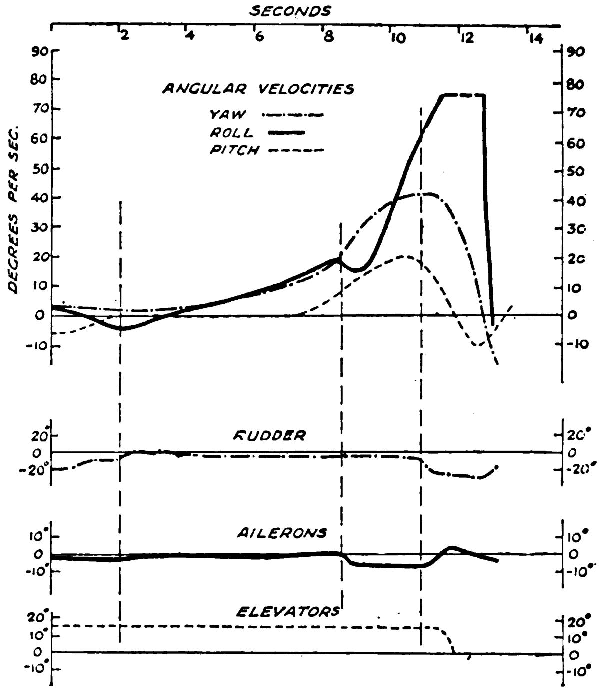

Fig. 5.—Failure of Ailerons to check a rolling turn.

in Fig. 3, where the amplitude of the roll is seen to increase automatically.

This particular machine at this incidence shows, therefore, two distinct forms of instability, one technically known as a divergence, which approximately doubles itself in every second, and the other an oscillation with a tendency to increase automatically.

Fig. 4 shows the effect of applying ailerons, in straight stalled flight, at about 20° incidence. For the first second after they are applied the aeroplane rolls in the direction to be expected, but almost immediately its direction of roll is reversed and it plunges wildly over on the opposite side.

Fig. 5 shows that this failure of the ailerons is even more marked when they are used to check a roll which has already started.

Fig. 6 shows that the rudder, applied in straight flight, has the desired effect of turning the aeroplane, but that it also causes it to roll in the sense that the wing which is being pushed forward rises.

Fig. 7 shows that the rudder can be used to check a roll which has already started, if it is applied so as to reverse the turn which accompanied the roll (i.e., to push forward the falling wing tip). The delay in the action and the violence of the reversed roll which follows should be noted.

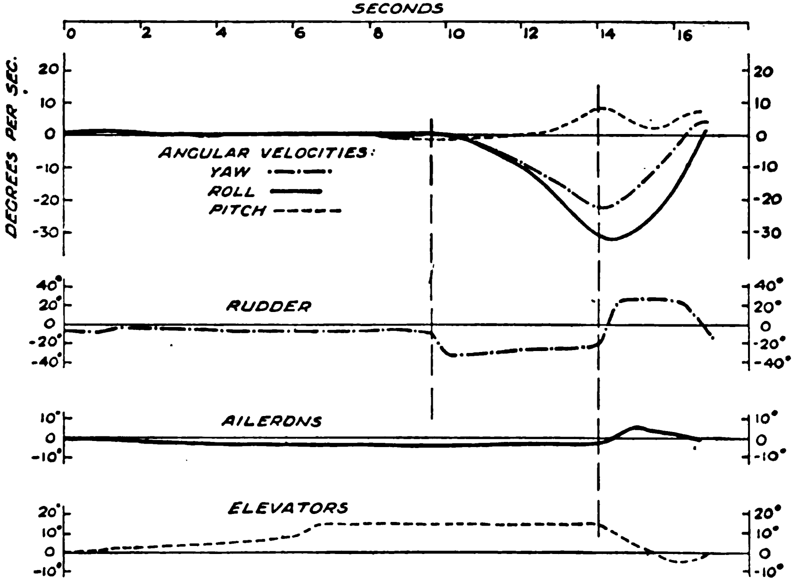

FIG. 6.—Effect of Rudder applied during steady flight. (Note indirect influence on rolling.)

Fig. 8, however, shows that if applied too late the rudder may, in certain circumstances, fail to check this unstable motion, though for the first two seconds after it was applied it appeared to be going to succeed.

Fig. 9 shows the ultimate history of an attempt to control these unstable motions by means of the rudder alone; note the liability of the oscillation to increase despite the pilot's efforts to check it.

These results have been chosen as being representative of the others, and because each shows some particular aspect of the matter clearly. In many of the other results these various aspects are so mixed together that a practiced eye is required to disentangle them.

The utter uselessness for any practical purpose of control with the above characteristics requires no emphasis.

As I have stated these results can all be explained in general terms by mathematical analysis based on data obtained from wind tunnels, and though the analyses are intricate the broad explanation can be given very simply.

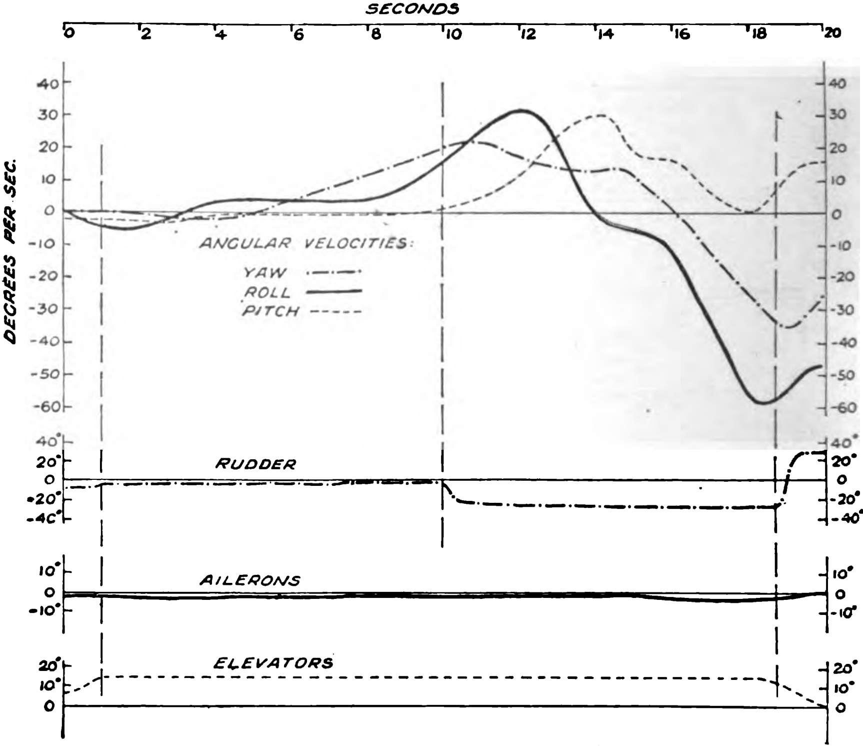

FIG. 7.—Rudder succeeds in checking a rolling turn. (Note delay in action.)

The primary cause of the trouble lies in the change of the effect of rolling upon the rolling couple. Instead of a large couple opposing the roll being generated, as in normal flight, a slight couple is generated in the sense to increase the roll. This is because (see

Fig. 1) the increased incidence of the falling wing tip no longer increases the lift upon it, but slightly decreases it. There is thus nothing but the inertia of the aeroplane to prevent rapid rolling. The complicated effects of inertia, such as those we observed at the beginning of the lecture in relation to ships, are thus introduced, and the valuable factor of time for the pilot to think is absent.

The instability of the motion is easily explained. When a stalled wing rolls there is not only a slight couple increasing the roll, but in addition a couple tending to retard the falling wing. This is because drag increases very rapidly with increase of incidence on a stalled wing. Now in stalled flight, even a small rate of yaw generates a large rolling couple depressing the retarded wing. This is partly because the advancing wing is travelling faster than the retarded wing and partly because of the side slip which follows the yaw. (It has been found that the rolling moment produced by side slip is, for some as yet unknown reason, very much greater on a stalled than on an unstalled wing). Any slight disturbance in yaw therefore generates a rolling couple, and the resulting roll both tends to increase itself and

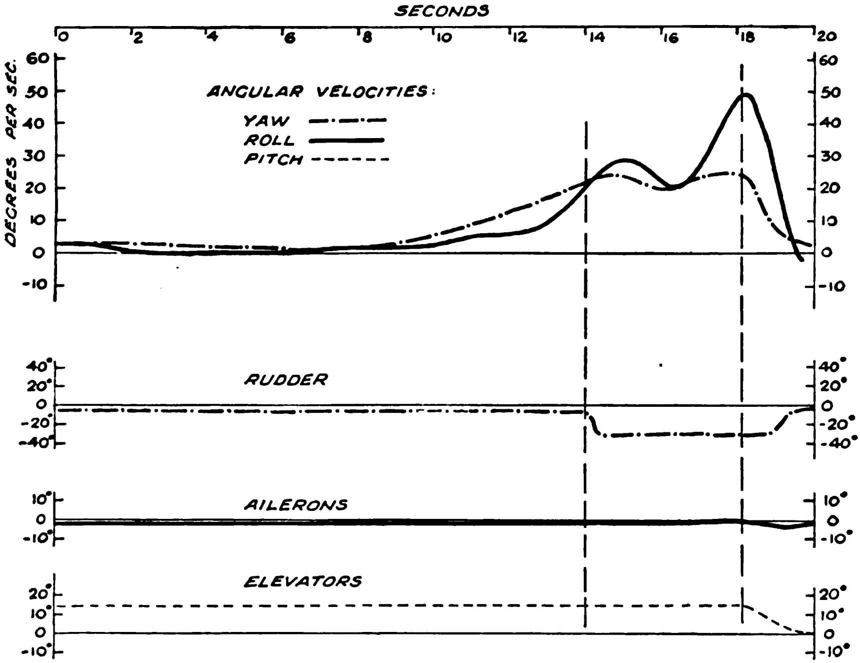

FIG. 8.—Rudder fails to check a rolling turn.

to cause the rate of yaw to increase still further. The process is obviously cumulative.

The increasing oscillation can also be explained, but the explanation is too involved to be given in the time at my disposal.

The outstanding feature of the whole problem is, however, the great influence of yawing motions on rolling couples, and this should be borne in mind when the action of the controls is considered.

The characteristic feature of aileron control in stalled flight is that, whilst the rolling couple which they can exert is weak, they also exert a powerful yawing couple retarding the wing tip which

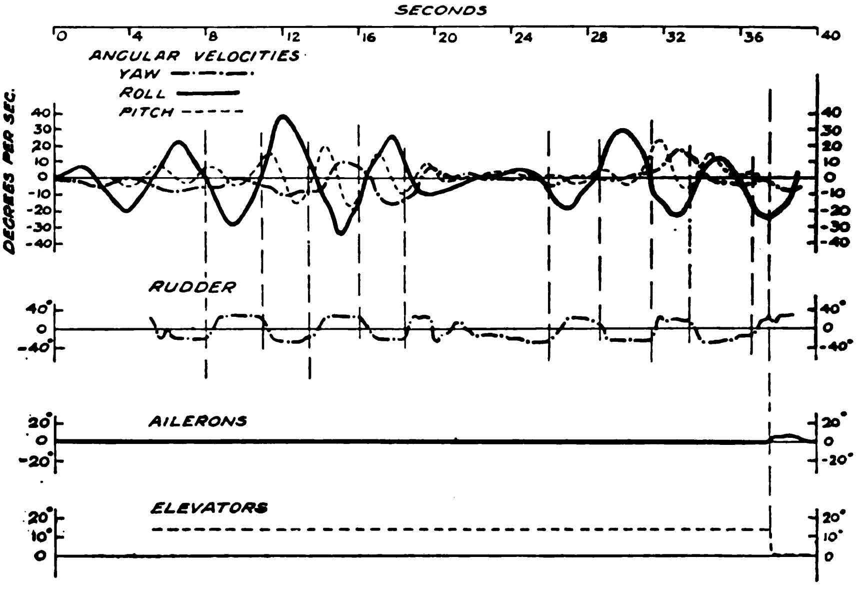

Fig. 9.—**Attempt to control the roll by means of rudder alone.**

they are trying to raise. This starts the aeroplane yawing, and a large rolling moment is soon indirectly generated by the yaw which entirely swamps the direct action of the ailerons. The apparent success, in Figs. 4 and 5, of the ailerons, for the first second after their application, followed by complete failure, is thus explained.

The action of the rudder in causing the aeroplane to roll so that the retarded wing falls is now easily understood. The difficulty of using the rudder in a way which will not cause an increasing oscillation arises from the fact that its effect on roll is delayed until it has succeeded in generating a yaw. We saw in the beginning of the lecture that delay in the control of a motion, which automatically tends to increase, is fatal.

To sum up. The motion is violently unstable in two ways. The ailerons are a positive source of danger, since they produce an effect which at first appears satisfactory but is ultimately the opposite of that expected. The rudder, though effective, provides a kind of control very difficult to use and, moreover, is often too weak even to prevent the first unstable plunge from continuing.

The continuous series of accidents resulting from accidental stalls near the ground are thus explained, for, though the motion can always be checked by thrusting the stick forward, and so diving to regain speed, this remedy is of no use when the ground is near. The pilot's reflex response when he finds himself suddenly rolling over and diving into the ground is to pull the stick back and to the side against the roll with, as we have seen, disastrous results.

I hope that I have now made the causes of the trouble sufficiently clear.

There are two lines along which a cure can be affected. One is to eliminate the instability and the other to improve the control. If I have succeeded in conveying my argument properly you will realise that the final solution will be along both lines.

The simplest way of improving the control was that originally used by the Wrights: to supply a powerful rudder. This prevents the worst consequences of stalling, but it does not, as we have seen, provide a satisfactory control. Moreover, it can only cause the aeroplane to roll after it has started it yawing, and this may be very undesirable if there happens to be a house or tree on the side towards which the yaw has to be made. An effective rudder, however, though not a sufficient cure, is a necessary factor in any complete cure.

Another solution is to provide some form of control at the wing tip, which will powerfully lift the wing and simultaneously push it forward, rather than push it backwards, as does the standard aileron. The yawing action of the aileron will thus indirectly reinforce the direct rolling action, instead of opposing it as at present. There are several ways of doing this, one is to provide a surface beyond the wing tips which, even when the main wings are stalled, will be inclined downwards so as to meet the air edgeways. This surface will not be itself stalled, and if its angle is controlled by the pilot it can be made to exert either an upward and forward or a downward and backward force, which is just what is required. Such surfaces will also prevent rapid rolling and eliminate the instability. This device is used in Hill's Pterodactyl or tailless aeroplane. It is there signally successful; the Pterodactyl is said to be as stable and controllable when stalled as in normal flight.

A second method of achieving the same result is to place in front of the wing tips one of Handley Page's slots. If these slots are left permanently open they will delay the stall on the tips until long after it has occurred on the remainder of the wings; the characteristics of unstalled wings which prevents rapid rolling from occurring will thus be retained, and the tendency for rolling to cause yawing will be eliminated. Stalled flight then becomes very stable and easily controlled, though the rate of control is not rapid. If in addition the slots are interconnected with the ailerons so as to close on the side to be depressed, but to remain open when the stick is central, the power of rapid control wiil be secured in addition to stability, for the closing of the slot will cause the wing tip to stall, with a consequent large loss of lift and increase of drag, by comparison with the other tip on which the slot remains open. The large direct rolling moment generated in this way is thus reinforced by the indirect effect of the yawing moment resulting from the increased drag.

Unfortunately the slots cannot be left open permanently, because the wings will then absorb too much power in normal flight, hence schemes have had to be devised to cause them to shut automatically, as the incidence falls below the stalling angle. Handley Page has devised one very successful method of doing this, of which you will have heard in the Press. Another somewhat different method of doing the same thing has been devised by McKinnon Wood at Farnborough.

Which of all these methods will ultimately prevail is as yet uncertain, but that a complete cure in a practical form can be found is now beyond doubt, and we can confidently look forward to a time when, its principal danger having been eliminated, flying will be ready to take its place in the world's transport on terms which, in respect of danger, will compare not unfavourably with the older and more established methods.

[M. J.]

WEEKLY EVENING MEETING,

Friday, February 17, 1928.

Colonel Sir Henry G. Lyons, D.Sc. F.R.S., Manager,
in the Chair.

Rev. E. M. Walker, LL.D.,
Pro-Provost of Queen's College, Oxford.

The University: Its Ideals and its Problems.

I think that you will agree with me that no apology is needed for the choice of my subject for this evening's lecture. The multiplication of universities, and the increase of the number of students in the universities, are among the most remarkable features in the recent history of education, both in this country and in the United States of America. In this country there were in the middle of the last century only four universities, in addition to the four ancient universities in Scotland; at the present moment there are no less than twelve universities, besides three university colleges which aspire to become universities. When we turn to the United States, it is hard to say which is the more remarkable—the increase in the number of universities and colleges of university rank, or the increase in the number of students in those institutions. The subject is of practical, as well as of theoretical, interest; for there are a number of questions which call for an answer, and these questions are, in effect, the problems of which I propose to speak. It must be evident that between the problems and the ideals there is the closest connection; for it is our conception of the ideal at which a university should aim which must determine our attitude towards the problems.

The Ideals.

I propose to take as my starting-point a course of lectures delivered in Dublin in the year 1852 by the late Cardinal Newman. His lectures on *The Idea of a University* is a book which successive generations of teachers in my own university have put into the hands of their pupils, as a model at once of clear thinking and pure English. It would be difficult to find elsewhere a finer example of the power of sustained analysis. It is here, too, that Newman's style is seen at its best; there are passages that haunt the memory with their matchless beauty.

Let us start, then, with Newman's definition of a university. He defines it as a place of *teaching* universal knowledge ; its object being the diffusion and extension of knowledge, rather than its advancement. The definition has thus both a positive and a negative aspect. On the negative side, the advancement of knowledge (i.e. the prosecution of research and discovery) is ruled out, as forming no part of the *essence* ; at best, it can only be a *property* or *accident*. On the positive side, the definition insists on teaching, as opposed to mere examining, and on residence and intercourse, as opposed to mere attendance at lectures. However much Newman's definition may be criticised, it is only fair to remember that it has the support of the Oxford Dictionary. In the dictionary a university is defined as " The whole body of teachers and scholars engaged, at a particular place,. in giving and receiving instruction in the higher branches of learning, . . . forming an institution for the promotion of education in the higher branches of learning." Here, as in Newman's definition, the advancement of knowledge forms no part of the essence. It is this negative part of Newman's definition that will come as a shock to the mind of the present generation. In many quarters the accepted view seems to be that research and discovery, i.e. the *advancement* of knowledge, are the main objects of a university. What then was Newman's grounds for the exclusion of this advancement of knowledge from his conception of the proper function of a university ? " If its objects were scientific and philosophical discovery," he says, " I do not see why a university should have students." He then goes on to argue (1) that teaching and discovery are distinct functions, and that, while teaching is the function of a university, discovery is that of an academy, or learned society, such as the Royal Society ; and (2) that the greatest minds have generally sought seclusion ; and (3) that down to his time the greatest discoveries, even in the Natural Sciences, had not been made in universities. In justification of what may seem to-day a paradox, it should be remembered that the greatest name in the history of scientific discovery in the nineteenth century is that of one who had no official connection with a university, and that he, like the other great minds of whom Newman was thinking, worked in seclusion. It should further be remembered that two of the universities which in the last half century have played a leading part in the advancement of knowledge, Berlin and Munich, both incorporated at the time of their establishment a previously existing academy : i.e. that the tradition which they inherited was that of an academy, and not that of a university. Newman and the Oxford Dictionary are so far right in their exclusion of the advancement of knowledge from the definition of a university in that a university is clearly not essential to research, and, if we may not venture to say that research is not essential to a university, we can, at least, claim that research is not as essential to a university as teaching. Where discovery is due to individual genius, it may well be independent of a university. The advantage of a university to the researcher is that it secures organization; i.e. the co-operation of the disciples with the master. We have, however, to consider, not only the advantage of a university to research, but the advantage of research to a university. Clearly, that advantage is to be found in the inspiration which research gives to teaching. In the region of higher learning the teacher is of little account, if he is not himself a learner. It is the prevalent conception of research and discovery, as forming part of the essence of a university, that is responsible for some of the most important problems that confront us, such as those of the functions of the Professoriat and of the institution of Research Degrees. As to the former of these problems, I would only point out that, while all, or almost all, universities impose on the professor the obligation of delivering so many lectures in the course of a term, and in so doing lend their support to Newman and the Oxford Dictionary, yet in the choice of a professor it is the aptitude for research, rather than the capacity for teaching, that determines the appointment. Most of us can call to mind professors who have been in the first rank as discoverers, and yet have been among the worst of lecturers. May it not be argued with some plausibility that, if a professor is to teach, he should be chosen for his power of teaching; and that, if he has no gift of teaching, it is merely stupid that he should waste his time on work that he is bound to do badly?

When we turn to the positive side of the definition, we find that there are three points that are insisted on. In the first place, teaching, rather than the mere conduct of examinations, is the proper function of a university. Secondly, residence, rather than the mere attendance at lectures, should be the ideal. Thirdly, education, rather than the mere acquisition of knowledge, should be its aim. Let us take each of these three points in turn.

When we come to consider the first of these contentions, that teaching, rather than examining, is the true function of a university, it is as well to remember that when these *Lectures* were delivered the great question at issue was that between the ideals of the older universities (Oxford, Cambridge, and Trinity College, Dublin) and that of the one modern university, the University of London. The University of London, which at the time of the delivery of these *Lectures* had enjoyed barely a quarter of a century of life, was the one university of a new type, and its avowed ideal was the examination system. On this count, the verdict is gone in favour of Newman, and it is an all but unanimous verdict. An examination in a multiplicity of subjects, of a multiplicity of students, educated under a multiplicity of conditions, conducted by a multiplicity of examiners who have no personal acquaintance with the students, and who have had no part in teaching them, is an ideal which would appeal to few minds to-day. It is felt in many quarters that it is a serious handicap to the university colleges that the examinations which are taken by their students are not under the control of the colleges themselves, and that the professors of those colleges have not necessarily any part in their conduct. My own experience as an examiner, and I may claim to have had some experience of examinations of very different kinds, has led me to the conclusion that the more closely the examiners are in touch with the candidates, the more they have had to do with teaching them, and the better they understand the conditions under which they have studied, the more satisfactory the result is likely to be.

To turn to the second point. In the middle of the nineteenth century, all the universities then in existence in this country, other than the University of London, were based on the principle of residence as Newman understood it; that is, on a life lived in common; and this held good of what was then the only university in Ireland. At the present moment, residence in this sense is not insisted on in the vast majority of the universities of the English-speaking world. It is mere attendance at lectures, rather than residence *intra academiam*, that is the indispensable condition. For all that, it is significant that this principle of residence is beginning to find favour in quarters where one would have least expected it. It is something, perhaps, that in our older universities this principle should have survived three university commissions; but it is much more significant that some of our modern universities and university colleges are tending in this direction. Unless I am much mistaken, our modern universities and university colleges are likely in the near future to fall into two different groups. There are those which either already insist on residence in a hostel attached to the university, or are more than half way on the road to this goal; and there are others of which it may be safely predicted that they will never be able to enforce such residence on all their students. It is true, of course, that a hostel, or hall of residence, is something very different from a college at Oxford or Cambridge; but the principle involved in the hall of residence, equally with the college, is one and the same—that of a life in common. What is really surprising is, that the introduction of our college system should at the present moment be contemplated by some of the leaders in the movement for university reform in the United States of America. It is evident at the first glance that peculiar difficulties must attend the introduction of anything like an Oxford or Cambridge college into an American university, where it would lack the glamour of tradition, and where it would have to compete with an already existing unit of sentiment, the Class. What is perhaps imperfectly apprehended on the other side of the Atlantic is, that the college is something more than a place in which undergraduates live, and move, and have their being in common. It is an autonomous unit, and it means a common life for dons as well as undergraduates, and traditions which owe their permanence to influences more enduring than those of the short-lived generations of undergraduates.

It was Newman's insistence on the principle of residence that led him to enunciate his famous paradox, "If I had to choose between a so-called university, which dispensed with residence and tutorial superintendence, and gave its degrees to any person who passed an examination, and a university which had no professors or examinations at all, but merely brought a number of young men together for three or four years, and then sent them away, as the University of Oxford is said to have done some sixty years since, if I were asked which of these two methods was the better discipline of the intellect, I have no hesitation in giving the preference to that university which did nothing, over that which exacted of its members an acquaintance with every science under the sun." It is easy to see what are the social advantages of the college system. The question, however, with which we are concerned at this moment is not what are its advantages as an instrument of social education, but what are its advantages as a discipline of the intellect. Put shortly the answer is, the opportunity that it affords for the interchange of ideas. This would be generally admitted in the case of the students. As Newman puts it: "When a multitude of young men, keen, open-hearted and observant, as young men are, come together and freely mix with each other, they are sure to learn one from another, even if there be no one to teach them." But even where there is some one to teach them, a teacher who is gifted and inspiring, there is a stage in intellectual development, in the study of some new and difficult subject, at which the young can learn more from each other, from their actual contemporaries, or from the man who is just one year their senior, than from the professor or the tutor. No drawback can be greater to a student than to feel that he is alone, that there is no one of his own age and standing with whom he can discuss and compare, no one against whom he can measure his own capacity. The advantages of the college system to the teachers themselves, the Faculty, is not so generally recognised, but it is not less real. Of the seniors, even more than of their juniors, it holds good that "the conversation of all is a series of lectures to each." It is difficult to compare what one learns from books with what one learns from conversation, and when one is laying the foundations of one's knowledge books are all important; but, speaking for myself, I think that it is no exaggeration to claim that what I have learnt from the conversation of others in the last thirty years of my life is hardly less than what I have learnt from books. It is here that I venture to think that a university planted in the midst of a big town is at a serious disadvantage. The intercourse of the members of the Faculty in a place like Göttingen or Jena is likely to be much more frequent and intimate than in Berlin or Munich, in Oxford or Cambridge than in Liverpool or Manchester. Nothing surprised me more in a visit to the American universities than to find that so little importance was attached to a common life among the members of the faculty. I call to mind an institution, the foundation stone of which had been laid a few years before in the virgin soil of the prairie, some half-a-dozen miles away from a busy commercial town. There was any amount of land to build on, and any amount of money with which to build. There was thus a great opportunity for a new adventure. Here, in the very midst of the college, within the campus, quarters might have been provided for the professors, whether married or unmarried. In a word, here was the opportunity for a common life. It was thought better policy to scatter the professors about in the neighbouring town—as missionaries, I presume, of the university's cause. Such propaganda is not likely to be successful, and even if it were successful, the sacrifice of the common lifer is a heavy price to pay.

Newman's third position, that education rather than the mere acquisition of knowledge should be the aim of a university, is closely connected with his second. By education he means a liberal education, and by a liberal education he means the acquisition of what he calls the philosophic temper of mind. This philosophic temper of mind, which is arrived at by an *enlargement* of mind, is in his view the direct result of that intellectual intercourse which is only possible in a university, and he defines it thus: "That only is true enlargement of mind which is the power of viewing many things at once as one whole, of referring them severally to their true place in the universal system, of understanding their respective values, and determining their mutual dependence." No higher ideal than this can be conceived of, and the ideal has never been more finely expressed. It must be remembered, however, that to Newman the instrument for the enlargement of the mind was the deductive method. One of the greatest revolutions in the intellectual history of mankind, a revolution effected in the course of the last century, has been the substitution of the inductive for the deductive method in reasoning. A hundred years ago the test of an educated mind was that it worked deductively; to-day, the test is that it works inductively. It is a revolution, by the bye, which we owe at least as much to the influence of academies and solitary workers as to that of universities and their professors. But it is still true that the proper business of a university is to impart the enlargement of mind, even though the method by which the mind works is no longer the same. It is surely a fact of common observation that many of those who have been trained, and highly trained, in inductive reasoning in their own particular branch of study are incapable of applying its principles universally, i.e. to subjects outside their own special science.

There remains one further ideal not touched upon by Newman, the ideal of freedom. Of this ideal there are two aspects. There is freedom in the sense of the autonomy of the university or college, and there is freedom in the sense of the liberty of thought and teaching. To deal very briefly with each of these :—

The autonomy of the university and of the college is one of the most highly prized of the traditions of Oxford and Cambridge. For centuries past the university and the colleges in either seat of learning have enjoyed perfect liberty to manage their own affairs. It is true that successive commissions have restricted this liberty, chiefly in the direction of augmenting the power of the University in relation to the colleges. For all that, the autonomy of the university and the colleges remains. Within the lines laid down by the successive commissions both the university and the colleges can administer their own affairs without control or interference, and what is all important, the Governing Body consists in the one case of all the members of the university engaged in teaching and administration, and in the other of all the Fellows of the college. Both in the university and the college the form of government is democracy in the strictest sense. The youngest Master of Arts in the university and the junior Fellow in a college have an equal vote and an equal voice with their seniors. I can imagine no experience more illuminating to a professor in an American university, in which he would have little influence, direct or indirect, over policy and administration, than that he should be suddenly introduced to a meeting of the governing body of one of our colleges, and were to hear some proposal of the Head of the college criticised with the utmost freedom by the junior Fellow. Doubtless there are difficulties of various kinds in applying this principle of autonomy to all universities, and university colleges. A university college, for instance, may not yet be ripe for a system which could be made applicable to a university ; but, for my own part, I am prepared to contend that the fuller is the control of the whole policy and administration of a university that can be given to those who are responsible for its teaching, the better it will be for the life, and the intellectual progress, of the university. At least, I trust that in this country the financial assistance of the Treasury will not involve any claim on the part of the Government, either to direct policy, or to interfere with curriculum. As to the second aspect of the ideal of freedom, the liberty of thought and teaching, I think that it may be presumed that it is an ideal which few in this country would be prepared to call in question. In the past the danger was that of theological intolerance. If that is no longer even a bogey, there are other intolerances that might easily prove fully as injurious to the enlargement of mind, and to the progress of research. At this moment the danger is more apparent on the other side of the Atlantic, where it is alleged that these other forms of intolerance have affected the appointment to, or tne tenure of, professorial chairs, and that on more occasions than one, and in more universities than one. I gather too that in America freedom of thought, or at least of the expression of one's views, is not as unrestricted as it is among ourselves. It is probable that Socialism in some form or shape is the subject of discussion in debating societies, almost every term, in almost every one of our universities. I have been given to understand that it is a subject that is systematically excluded from the *agenda* of such societies in the universities of the United States.

THE PROBLEMS.

First in importance among problems is the question of size, that is, of limitation of numbers. In our country this problem is not of such urgency as in America; but even here it is a problem that confronts Oxford and Cambridge, and possibly not these two alone. The question is often asked, Should any limit be set to the number of students in a university, and, if so, on what principles should it be determined? It is clearly a more urgent problem for universities based on the college system than for those which do not insist on residence in this sense. At Oxford, at any rate, it is very generally felt that colleges must extend their buildings, if the present numbers are to be maintained. That all Freshmen should have rooms in college, and that every Scholar should have a right to reside in college for three years, and every Commoner for two, is a principle on which there would be found to be, I fancy, a large measure of agreement. The only question that arises is how soon colleges can afford to provide the necessary accommodation in view of the enhanced cost of building, which may be put at two to three times its cost before the War, at any rate if stone is the material employed. It is generally assumed in America, even in the best informed circles, that the college system in our older universities means that all our students reside in college for the whole of their undergraduate course; and I am not surprised that they should be disposed to regard the claim that our system is based on the residential principle as ill-founded, if in reality the majority of our undergraduates reside in college for less than half their time. It is in America, however, that the problem of size has come to be regarded as the most urgent of all their problems. When I was in America, less than ten years ago, I visited a university in which the number of students had grown, in something like a dozen years, from three thousand to eight thousand: a fact of which the authorities seemed proud. I suggested that at this rate the numbers might rise to twenty or thirty thousand students at no very distant date, and I asked whether a university of 30,000 students could be worked. It seemed to me that the question that I put had not occurred to those in authority, or at least had not been considered seriously by them. The enormous increase in numbers in most of the universities of the United States since the date of my visit has compelled even the most sanguine minds to recognise that a problem is involved. It is one with which a privately endowed university is perfectly free to deal, on any principle, and in any manner, that it pleases. Three of the most famous of such universities—Harvard, Yale, and Princeton—have, as a matter of fact, arrived at an understanding as to the number of students to be admitted in each year, and the effect of this understanding is to reduce in no slight degree in each case the number of those included in the College of Liberal Arts. For the State Universities the problem is not so easy of solution. How, it is asked, can an institution which is supported by the state out of the taxes which it levies, or out of the other resources at its command, refuse to receive any citizen of the state who seeks admission? What right has the university to exclude him? It was the war that convinced the American mind of the superior efficiency of the college man or woman; hence at the present moment to enter a college or university is an ambition natural to all. That some limit must be set to the numbers, that the numbers must not only be reduced, but reduced very seriously indeed, is a view that is winning more general acceptance in America each year that passes. The disadvantages attending on the present numbers are obvious enough. Lecture rooms and laboratories are overcrowded; there is little possibility of contact between the teachers and the taught; and, what is worst of all, a large proportion of the students disappear, from want of means or want of brains, at the end of their first year. It looks as if the solution of the problem might be found in the institution within the university of two separate departments, a junior and a senior; the former preliminary in character, for students in their first two years, and the latter for the more advanced work of the two later years; for those, a comparatively small number, who had succeeded in qualifying for admission.

To turn next to the problem of finance. This is a problem which in this country has arisen in the main from the demands of science. So far as Oxford and Cambridge are concerned, there would be no such problem if science, and the teaching of science, stood where they did at the end of the last century. New branches of science are being introduced into the curriculum, and this involves ever increasing expenditure. There is one point in Newman's definition on which I have not yet touched, but which has a very direct bearing on the question of finance. He defines the university as a place of teaching universal knowledge. This part of his definition is based on a misunderstanding of the word *universitas*, which, so far from connoting universal knowledge, means simply a body corporate; a corporation of Masters and Scholars. No one nowadays would venture to maintain the old view of the meaning of the word *universitas*, but the idea still persists that the teaching of universal knowledge is the proper function of a university. No university in this country professes to teach every branch of knowledge, but it is undoubtedly the ambition of all to teach as many subjects as possible.

Whether this is wise may well be doubted. Experience seems to show that some universities are better suited for the study and teaching of certain subjects, and others for the study and teaching of other subjects, and that the success of a university in teaching such subjects depends much more on the spirit and tradition of the place than on the expenditure of funds. Is it too late in the day to plead that universities should come to some sort of agreement among themselves—that they should accept some rationing of subjects? It is far better for a university to concentrate its resources and its efforts on the teaching of those subjects which are for it the most important, rather than to dissipate both energies and resources in the attempt to cover the field of knowledge. The question of finance has an intimate connection with that of autonomy. The State Universities of America, as has been pointed out already, are dependent, some of them wholly and others mainly, on the appropriations voted by the State Legislatures, and this being so, it must be difficult to exclude the influence, direct or indirect, of these legislatures on the policy and administration of the universities. In order to guard against the danger of a chance majority in the State Legislature having it in its power to impose a policy on the State University by the threat of a reduction in the annual appropriation, the practice has been adopted in some of the middle Western States of introducing into the constitution itself of the State a provision that the appropriation should not be reduced below a given minimum. Although a revision of the constitution is a favourite pastime in certain of these States, the interests of the university are at least safeguarded until the next revision is called for.

Some sort of a parallel to the State Universities of the U.S.A. may be found in those of our modern universities which receive financial aid from the local authorities. Let us hope that the recent action of the local authorities in one of the great industrial towns of the North in regard to secondary education may afford no sort of precedent for a similar policy in regard to universities.

The last problem on which I wish to touch is one which chiefly affects the universities of the modern type and the university colleges. For obvious reasons, the two older universities are little concerned with it. It is a fact, I think, that in all our university colleges, and in many of the modern universities, the bulk of the students on the Arts Side, as distinguished from the Science Side, are in receipt of grants from the Board of Education. Hence in such colleges and universities the Professor of Education is recognised as being not the least important member of the staff. It is a condition of such grants that the recipients should pledge themselves to enter the teaching profession, and to teach in an elementary or a secondary school for a given number of years. In the past it might have been argued that the Board of Education has no right to spend public money on the university education of others than those who intend to enter the profession of teaching. In view, however, of the institution of State Scholarships this argument has lost much of its force, and against this policy two very grave objections may be urged. In the first place, nothing can be worse for the development of the Arts Side of these institutions, whether colleges or universities, than that so large a proportion of the students should be preparing for the same profession, and that a profession of a very special kind, and in some cases at least with a very narrow outlook. Here Newman's argument comes in with all its force. What our modern universities and our university colleges desiderate on the Arts Side is precisely that coming together of a multitude of young men, keen, observant and sympathetic, as young men are, with different studies, different interests, and different careers before them, and that is precisely what, under present conditions, they too often fail to secure. In the second place few things are better calculated to diminish a student's interest in his work, and to depress his energy, than the burden of a pledge. If he is pledged to enter a particular profession, and if he knows that if he gets a degree of any sort or kind a post is waiting for him in that profession, the last thing that he is likely to feel within him is the thirst of knowledge for its own sake. Public opinion in our universities has long since condemned the practice of exacting a pledge to take Holy Orders from boys who have just left school. To some of us the teaching profession is hardly less sacred than the clerical. At any rate, there must be few of us who have not cause to remember what may be suffered at the hands of a teacher who has no heart for teaching. Has not the time come for ceasing to exact a pledge from those who are to enter the teaching profession ? There can be no question that it would be infinitely better for the intellectual life of the universities and colleges, and infinitely better for the intellectual interests of the students themselves, if those who receive the assistance of public money were free from any pledge or obligation. How far it would be possible to convert the Board of Education grants into State Scholarships, and to trust to the inherent attractiveness of the teaching profession to secure a sufficient number of recruits, I am not in a position to determine. I have reason to believe that there are many besides myself who hold that the ultimate solution of this problem can only be arrived at on some such lines as these.

[E. M. W.]

WEEKLY EVENING MEETING,

Friday, March 2, 1928.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S.,
Vice-President, in the Chair.

SIR FARQUHAR BUZZARD, M.D. F.R.C.P., Regius Professor of Medicine, University of Oxford.

The Psychology of the Sick.

THE title of my discourse to-night is one for which I must disclaim all responsibility. It was suggested by your Treasurer, Sir Arthur Keith, in a moment of alliterative inspiration and accepted by myself, as lawyers say, without prejudice, on account of its non-committal character.

In these circumstances the by-law of the Royal Institution, under which the lecturer is obliged to confine himself to the subject-matter of the particular branch of knowledge he is engaged to lecture upon, may, I hope, be given an elastic interpretation.

This preliminary and premonitory explanation seems desirable because "The Psychology of the Sick" is a phrase open to misunderstanding. It may, for instance, have lured you here in the secret hope of obtaining gratuitous advice in regard to the humouring of invalid, perhaps peevish and difficult, relatives, or possibly of receiving hints on the mental attitude which it is most propitious to adopt towards a prospective Channel crossing.

Should this be the case you are doomed to a feeling of disappointment, which I trust will fade into the background while I attempt to indicate as briefly as possible the real object I have in view.

Whether at any period in history perfect health was universal it is impossible to say, but there can be little doubt that the occurrence of ill-health has been for thousands of years one of the chief motives behind man's desire and man's search for knowledge concerning himself, his structure, and his functions. In other words, the desire to combat disease has been largely responsible for what insight we have obtained into the anatomy and physiology of our various organs. It may be questioned whether mere curiosity, without the stimulus of ill-health, would have afforded the necessary incentive; whether, for instance, we should not still be ignorant of the composition and activities of the gastric juice, had not indigestion marred the happiness of countless lives.

We may go further and say, without fear of contradiction, that disease has not only *prompted* research, but has provided man with much of the material essential for his investigations. At every step in the progress of biology careful observation and consideration of the abnormal have given us the means by which our conceptions of the normal could be controlled and checked. And thus it has come about that the anatomist who studies healthy structure, the physiologist who studies healthy function and the pathologist who studies unheathy structure and function, working on separate but converging fronts, with numerous lines of inter-communication, have amassed an immense quantity of information concerning those organic mechanisms which constitute the manifest mainsprings of human life and human action.

One corner of this vast field of research has been for many centuries the resort of students intent on disclosing the secrets of man's mentality. Watered, from time immemorial, by the speculations of philosophers and psychologists sitting on its fence, its soil, in more recent years, has yielded an abundant harvest of ripening fruit in response to the untiring labours of anatomists, physiologists and pathologists.

To employ another metaphor, the problem of understanding the mechanism underlying mental processes has been, and is being approached by many avenues, from each of which the common but distant goal wears its own distinctive aspect, and in each of which may be heard a language fully comprehensible only to its habitual occupants.

Hitherto the study of conduct and behaviour, as the expressions of mental activity, has been regarded as the special province of the psychologist, but evidence is accumulating to show that it must depend in the future more and more on the co-operation of workers in other branches of science.

It is my purpose to-night to submit for your consideration some of the facts which pathology has to offer as her share in this co-operative undertaking, and to enlist the services of anatomy and physiology where and when they are necessary or helpful to their understanding. Let it be understood that I am using the word "pathology" in its widest sense, the scientific study of disease from both its bedside and laboratory aspects, the chief object of which is to correlate disorders of function with changes in structure. In physical disabilities this correlation is so familiar and obvious to us all that it needs no elaboration for its acceptance—the proper function of a joint, that is to say its mobility, is disturbed by changes in its structure, the result of a disease or of injury.

In the domain of psychical disabilities the pathologist is seeking to determine how far disorders of the mind can be correlated with changes in the structure of the brain. On the answer to that question our conception of the mind and its workings must to a large extent ultimately depend. Is our conception of the mind or soul, words used with little discrimination to denote man's intellectual and emotional activities, to conform to that of ancient philosophers who regarded it as an immaterial, indefinable, supernatural and disembodiable form of energy hovering around the heart, the liver or the head; or is it to be one which regards it as the live function of the brain, as intimately related to the structure of that organ as the circulation of blood is to the structure of the heart, or the digestion of food is to the stomach?

Before introducing to your notice the associated psychical and physical disorders attendant upon disease, let me remind you of certain salient features pertaining to the normal anatomical and physiological development of the brain.

As in the animal scale, so in the course of the growth of each human being, a close parallel is found between the degree of development of the brain and the degree of development of intelligence or mental capacity.

The new-born infant possesses a brain which is not only smaller but more primitive in structure than that of the adult. Examined by the aid of the microscope large numbers of the nervous elements of which it is mainly composed are seen to be immature, and therefore incapable as yet of taking their part in those activities for which they are destined. As months and years pass by more and more of these nervous elements become mature, with the result that millions of nerve cells, constituting the grey matter, are brought into physiological connection with each other by means of innumerable processes and their branches.

This developmental procedure is continued far into adult life, and corresponds with the increase of knowledge and expansion of intelligence characteristic of those years.

The complexity of structure and the multiplicity of nervous connections found in the brain at the prime of life are only comparable with the intricacies of perception, of memory, of thought and of expression of which the human mind is capable. The potential activity of this nervous field during working hours is beyond our powers of conception, but it has been likened to that of a huge building containing, in a vast number of rooms, the whole population of the globe, each member of which is provided with a telephone, a wireless head-piece and a typewriter, whereby messages can be exchanged and recorded with lightning speed and unerring accuracy between every floor and every department. I need hardly add that the author was probably using his own brain with a private, not a government, concern for the purpose of his simile.

After middle age the multiplication of processes and the formation of new links between various centres gradually ceases, and at the same time the mind becomes less and less capable of acquiring fresh knowledge or of making novel adjustments. Finally, with advancing years there comes a time when, although the memories of the past are still preserved, the events of to-day and yesterday make no lasting impression.

It is perhaps in the early period of life that the comparative study of the structure and function of the brain is most enlightening. From birth onwards every month witnesses structural changes affecting, in an orderly manner, one system or group of cells and their branches after another, with a result that each in turn becomes prepared for its appropriate activity. These changes can be compared with the progressive manifestations of various functions. In the new-born child movements are at first purposeless and irregular; muscular contractions take place without method or co-ordination. Reactions to light, sound and touch are present from the beginning, and those to taste and smell are soon added. At the end of a month the child smiles and the eyeballs follow a lighted candle, a few weeks later signs of perception and recognition can be detected, and at three months of age movements begin to show purpose. At six months there is evidence of some appreciation of space and distance, while the faculty of imitation makes its appearance. The first signs of co-ordinating muscular action for the purpose of gaining equilibrium in the erect posture follow towards the end of the first year, and at the same time efforts at talking become increasingly noticeable. And so the story goes on until the years of maturity and discretion are ultimately reached.

In these brief and general terms I have outlined the undisturbed life-history of a normal human brain and a normal human mind. We have duly traced their parallel development from their infantile immaturity to the zenith of their career, paused for a moment to wonder at the display of their fullgrown powers, and then recorded the inevitable changes which come with declining years.

But such an uneventful story falls to the lot of no human being, and we must turn our attention first to some of the agencies which disturb the stream of mental and moral progress, either temporarily or permanently, and later to the structural changes found in the brain in association with those disturbances. The number of such agencies is so great that it will be impossible in the time at my disposal to consider more than a few of them, and my choice will naturally fall on those with which we are most familiar and which most readily lend themselves to illustration.

Before discussing these pathological phenomena let me remind you of a principle, a natural law, to which, with few exceptions, they are subject. Those functions of the brain which are most recently acquired in time and most highly specialised or complex in character are the first to fail and the last to recover, if recovery is possible, when the organ is exposed to injurious agencies.

Let me illustrate this in the simplest possible way. A girl of fifteen has a long and exciting day and is very tired. She can sit and knit without dropping a stitch because she has knitted for years and the necessary movements have become practically automatic. On the other hand, she has only recently taken up the piano, and the new piece which she could play without a mistake when she was fresh this morning is quite beyond her powers to-night. Both the knitting and the piano-playing are acquired accomplishments involving the action of nervous centres in the brain, but in the one case the acquisition has been longer established and the mechanism is less highly specialised and complex than in the other. Or let me take another example: a boy of five or six years of age, well brought up so that he has learned, in common parlance, to behave nicely, goes through an attack of influenza associated with fever lasting perhaps for a week. While convalescent he is found to be unreasonable, irritable, and easily moved to anger and tears by trivial disappointments or thwarted desires. In this case, again, there is evidence that something recently acquired, something rather more highly specialised, has failed and left the primitive, emotional reactions, such as he displayed in infancy, bereft of control.

In both instances we recognise a *transient* change in mentality—the girl is tired and will know her piece to-morrow after a good night's rest. The boy is excused his behaviour because he has been ill, and we confidently expect him to be himself again in a week or two. Are we to be satisfied with these mere words as an explanation of the phenomena we have witnessed, or shall we inquire whether these temporary disorders of function are associated with any material changes in the brain?

So far we have referred only to two agencies capable of provoking slight but transient mental disturbances—fatigue, and a poison of bacterial origin circulating in the blood stream. We know that many other bacterial infections may produce similar and even more severe and more lasting results. We know, too, that other poisons, such as alcohol, even in moderate quantity, are often responsible for psychical disorders presenting identical features, and if absorbed in larger quantities by the adult will have proportionately greater effects. First of all, interference with mature judgment, reasoning power and memory, followed by impaired inhibition of primitive emotions and desires and then by reduced control over speech, articulation and other more physical activities. May I remind you, too, of another agency: the loss of an adequate supply of oxygen to the tissues brought about either under experimental conditions or by climbing or flying to great heights.

Haldane and Kellas confined themselves in a steel chamber, from which air was gradually pumped out, with the object of recording the effects produced. The latter were nearly disastrous because, when the supply of oxygen was diminished, they not only became incapable of making accurate observations, but their mental faculties were so reduced that they repeated the same answer to every question addressed to them and could not even stop the experiment. Fortunately, they were released in time, but two other enthusiasts took their place. In the course of this second experiment those outside the chamber had become more solicitous for the safety of those inside, and at one period let in some more air. This friendly act provoked an expression of resentment from one of the occupants, couched in language so far removed from anything possible to him in ordinary life as to indicate that his self-control and his knowledge of the fitness of things had completely vanished.

Again, Major Hingston, describing the physiological difficulties in the ascent of Mount Everest in 1924, mentions the effects of high altitudes on the mind. One member of the party was conscious of a dulling of will-power, a diminution of his strength of purpose as shown by less and less desire to reach the summit, the further the ascent progressed. Another described enfeeblement of memory and increasing inaccuracy of observations, so that they were forgotten or distorted unless they were at once recorded. Every member of the party experienced mental lassitude and disinclination for any kind of mental effort.

Ethical and merciful considerations make me hesitate to dilate upon such information as can be gleaned concerning the functions of the human brain when that organ is exposed to the influence of laughing gas or some other anæsthetic, commonly employed by medical men in the exercise of their profession. Suffice it to say, that during the process of what is generally termed "going under" the higher, more specialised, faculties are the first to fall victim to the circulating poison, and that sometimes the patient's language and behaviour combine to expose the underlying strata of his mental and moral fabric in no uncertain manner. Fortunately, the period of time which elapses between the first confusion of thought and insensibility to pain, followed by complete loss of consciousness, is as short as the anæsthetist's memory.

Examples illustrating the transient, evanescent influence of changes in the normal blood supply to the brain, such as insufficient oxygen, an excess of fatigue products, and the presence of poisons generated within or without the body, could be multiplied indefinitely. Enough evidence, however, has been adduced to show that through these agencies the intellectual and moral faculties of the mind can be profoundly altered. Incidentally, there has been provided an explanation of certain phenomena of behaviour quite different from that which was once generally accepted. Much abnormal behaviour and language, characterised as bad or unnatural, are not, generally speaking, evoked *actively* by the presence of evil spirits or other irritating agents; on the contrary, they are often the *passive* result of the temporary disorganisation of controlling or inhibitory mechanisms. In other words, they belong to the category of what, in physiological terminology, are known as "release" phenomena or in nursery phraseology "when the cat's away the mice will play."

This principle becomes more firmly established when we consider the case of children whose minds do not develop along normal lines and who are classed as congenital idiots or mental defectives. Their brains are frequently smaller than those of their healthy brethren, and even if equal in size exhibit differences in minute structure when examined under the microscope. In other words, the material basis upon which mental and moral attributes depend is either wholly lacking or completely inadequate. The brain and mind of the healthy child adjust and adapt themselves to their environment by a succession of physiological reactions taking place along definite anatomical paths, and unless these reactions are possible there is no development. In ordinary language, without a normal, healthy brain education and training by means of precept, example, encouragement, suggestion or punishment produce little or no results, moral or intellectual.

We have yet to consider the permanent effects produced by certain injurious agencies on brains and minds which have progressed far along healthy lines in the direction of maturity. No better illustration can be offered than that provided by the disease which goes by the technical name of Lethargic Encephalitis, and by the popular designation of Sleepy Sickness. Let me preface my remarks about this scourge of recent years by saying that, although it is sometimes fatal and often leaves lasting and disabling effects, it is an ailment from which complete recovery occurs in many instances.

Quite briefly, sleepy sickness is a disease of the brain produced by a germ which invades its substance and there creates havoc, partly by causing inflammation, and partly by desseminating poisons which injure and destroy nerve cells and nerve fibres. The results of this invasion are as varied as are the functions of the brain, because any part of that organ may be attacked. All forms of paralysis, blindness, deafness, loss of speech and of mental faculties are from time to time left in its train, but I am calling your attention to only one group of cases. This group not only presents features of interest in connection with the subject of my discourse, but has become a social and economic problem of no little importance to the community at large. It comprises a number of individuals of school age, anything from seven to seventeen years, who, up to the onset of their illness, have been bright, well-conducted children endowed with average or more than average ability and character. The disease has left them physically unimpaired, often superficially intelligent, but hopelessly lacking in moral sense and responsibility.

These children become quite impossible to deal with at home, at school or even in a hospital ward. They are restless, unstable and impulsive, exhibiting outrageous and even criminal propensities. At the same time their misdeeds do not appear to be the result of malice, they are perfectly aware of them and often express or show some remorse. Although their general intelligence is only slightly affected, they are not receptive, and their memory and power of concentration remain much below the average, with the result that they do not grow up or mature. Still worse, experience has shown that all efforts expended on re-educating their moral sense and responsibility, either in their homes or in institutions, are practically fruitless. It may be presumed that the peculiarities of behaviour characteristic of this disease as it affects many children are determined by the fact that it has supervened during the period of development. For this reason certain association paths and centres destined to exercise a controlling influence over lower, more primitive, centres have been nipped in the bud before they have had time to become firmly established.

The changes in mentality which I have described in association with fatigue, illness and other factors must be familiar enough to all of you, but I have suggested that they can be correlated with changes in the structure of the brain. It behoves me to support this suggestion by evidence more substantial than words, and for this purpose I must approach your minds along visual in addition to auditory paths. The evidence which I propose to submit is drawn chiefly from pathological, to a less degree from anatomical and experimental physiological sources, and time will not always allow me to specify its origin in more detail. If I succeed in giving you a general idea of its character and of the methods which science has pursued in disclosing it my object will be attained.

(At this point the lecturer showed a number of lantern-slides, illustrating both the normal structure of the brain, and such changes in its nerve elements as are found in association with fatigue, poisoning, inflammation and mal-development.)

In this attempt to demonstrate the relationship between disorders of mentality and changes in the structure of the brain, and to indicate the methods by which such changes in structure are exposed to view in a pathological laboratory, I have been content to utilise only a small fraction of the evidence at our disposal and of the knowledge which has accumulated in recent years. Its significance would have been much enhanced if I could have laid before you at the same time the results of recent *physiological* experiments bearing on the complex reactions of the higher nervous centres, and especially the brilliant work of Pavlow in relation to conditional reflexes. But pathology alone warns us that we cannot remember, recognise, reason or express ourselves unless our brains are structurally healthy and intact, and that there are material alterations of structure, physical or chemical in origin, underlying and perhaps even *determining* the Psychology of the Sick.

[F. B.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1928.

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Mrs. Eileen Isabel Dick,
Egerton John Glyn,
Leonard Vardey Haigh, C.B.E. M.I.Mech.E.
Walter Ambrose Heath Harding, M.A.
Harold S. Harron,
Mrs. C. D. Langworthy,
Philip de Laszlo, M.V.O.
Thomas Percy Legg, C.M.G. F.R.C.S.
Archibald Munn McCutcheon, M.B. Ch.B.
Philip Edward Marrack,
Peter Boyd Moodie,
John George Porter Phillips, M.D. F.R.C.P.
Mrs. Marjorie Isobel Mackenzie Potter,
Mrs. D. McN. Riddel,
James Edward Wilson,

were elected Members.

The Secretary reported the death of Sir Dyce Duckworth on January 20, and of Mr. Alexander Siemens on February 20, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to place on record their sense of the great loss that the Royal Institution and the Science of Medicine have sustained by the death of Sir Dyce Duckworth, Bart., M.D. Hon. LL.D.(Edinburgh), F.R.C.P.(London), Fellow of the Royal Society of Medicine, Consulting Physician to St. Bartholomew's Hospital and to the Seaman's Hospital, Greenwich; Medical Referee to the Treasury, 1904-10; Honorary Physician to King Edward VII.

Sir Dyce Duckworth was one of the most distinguished Physicians of his day. He was a Member of the Royal Institution for more than fifty years, and always evinced the greatest interest in its welfare. He served as a Vice-President and on the Committee of Managers.

The Managers desire to express, on behalf of their Members, their sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Institution has sustained by the death of Alexander Siemens, Past President of the Institutions of Electrical and Civil Engineers, Member of the Institution of Mechanical Engineers.

Mr. Siemens was a Member of the Royal Institution for over forty years. He served on the Committee of Visitors, and subsequently on the Committee of Managers, and from 1913–15 he was Honorary Secretary and a Vice-President. In all these offices his keen interest in the welfare of the Institution was abundantly testified by his ready help and counsel in the conduct of its affairs. He delivered four Friday Evening Discourses on: Theory and Practice in Electrical Science (1893); Cable Laying in the Amazon River (1896); New Developments in Electric Railways (1904); and Tantalum and its Industrial Applications (1909).

The Managers desire to express, on behalf of the Members, their sincere sympathy with the family in their bereavement.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Carnegie Institution of Washington—Differentiation and Specificity of Starches. By E. T. Reichert. 2 vols. 4to. 1913.

Biochemic Basis for the Study of Problems of Taxonomy, Heredity, Evolution, etc. By E. T. Reichert. 2 vols. 4to. 1919.

Nörlund, Professor N. E. (the Author)—"G. Mittag-Leffler." 4to. 1927.

Parker, W. Rushton, Esq., M.D. M.R.I.—The New Century Dictionary. 3 vols. 8vo. 1927.

Rahman Khan, Habibur (the Author)—New Theory of the Generation of Wireless Waves. 4to. 1927.

Travellers' Club, The Secretary—History of the Travellers' Club. By Sir Almeric Fitzroy. 8vo. 1927.

WEEKLY EVENING MEETING.

Friday, March 9, 1928.

His Grace the Duke of Northumberland, K.G. C.B.E.
M.V.O. LL.D., President, in the Chair.

Edward A. Milne, M.B.E. M.A. M.Sc. F.R.S.
Beyer Professor of Applied Mathematics, University of Manchester.

The Sun's Outer Atmosphere.

Astronomers are accustomed to divide the outer regions of the sun into four parts, (1) the photospheric layers, (2) the reversing layer, (3) the chromosphere, (4) the corona. To-night I wish to speak particularly about the chromosphere. But before doing so I should like to dwell a little on certain aspects of this fourfold division. Meteorologists make a similar subdivision of the earth's atmosphere. We have the troposphere, the statosphere, the conducting layers, the auroral layers, and so on. But meteorologists have at least one advantage over solar physicists—meteorologists know where the earth's atmosphere begins. It may leave off very indefinitely, but it certainly begins quite definitely—it begins at the solid and liquid crust of the earth. It is sharply bounded below. But on the sun, and indeed on any star, no such sharp base exists. Whether the sun is wholly gaseous, or whether with Dr. Jeans we suppose it to be ultimately in a liquid state in the far interior, we are at least certain that owing to the high surface temperature and the positive temperature gradient implied by the outflow of heat the entire outer layers, down to a depth much greater than the furthest depth we can see, are in the gaseous state. We therefore have no datum line for the base of the solar atmosphere.

It is true that as seen in the sky the sun has a sharp edge. But we have to remember that at the sun's distance one second of arc corresponds to 700 kms., and that a line of sight passing one second of arc inside the sun's limb traverses 64,000 kms. of the solar sphere. Thus the sharpness is to some extent illusory. We can, however, assert that above the level corresponding to the "sharp edge" the gases are practically transparent as regards their continuous spectrum, whilst below this level they are practically opaque. If we knew the intrinsic opacity (per unit mass) of the solar material we could calculate the pressure at which the transition from practical transparency to practical opacity takes place, for a line of sight nearly tangent to the sun. Assuming that general opacity arises from the ejection of photo-electrons, the various unknowns may be estimated. We find that the opacity of a column of given length varies as the square of the pressure and hence falls off rapidly outwards. It appears that practical transparency along a tangential line of sight occurs at a pressure of about 10^{-6} atmos. When we view the sun's surface normally, at the centre of the sun's disc, we see to a deeper level. Calculation shows that all but one per cent. of the light originates at depths where the pressure does not exceed 10^{-3} atmos. This change of pressure, 10^{-6} atmos. to 10^{-3} atmos., appears to take place in a range of depth of some 50 kms. These limits serve to define the "photospheric layers"—the layers within which originates the light of the continuous spectrum sent to us by the sun.

Superimposed on the sun's continuous spectrum is an absorption line spectrum. Some of these lines show in their fine structure reversals, but we may ignore these and state that for the undisturbed solar disc we have broadly speaking a spectrum composed entirely of absorption lines. We are not compelled to assume that the layers producing these lines are entirely exterior to the photospheric layers. Theory shows that provided the temperature decreases outwards, an absorption spectrum will be shown if the gas has a general coefficient of absorption and superimposed on this certain selective absorption coefficients associated with particular wave-lengths. Thus if the general coefficient of absorption remained definitely constant at all levels up to the sun's boundary we should still have a Fraunhofer spectrum. Actually, if we accept the photoelectric origin of the general absorption, the general absorption coefficient per unit mass decreases with the pressure, and thus as we pass outwards the general absorption coefficient becomes practically zero, whilst the selective absorption coefficients are still large. There is therefore a region effectively transparent except in the lines themselves, Nevertheless, within the photospheric range of pressures already mentioned, selective line absorption will also be occurring. The term "reversing layer" is used to denote in a general way those layers which contribute to the Fraunhofer spectrum, but we now see that there is no precise delimitation between the reversing layer and the photospheric layers. The two shade into one another. The photospheric layers are also giving rise to line absorption, though this absorption will be weak ; in other words, the residual intensities of the lines produced in this region will not be much below the intensity of the continuous background. The upper reversing layer, transparent except in the lines, will give rise to stronger lines, i.e. lines with smaller residual intensities.

As evidence for this we have that the stellar sequence of spectra indicates a pressure of the order of 10^{-4} atmos. for the layer in which absorption lines of excited atoms originate, but a pressure of the order of 10^{-7} atmos. for the layer in which absorption lines of normal atoms originate. The former pressure lies inside our photospheric range of pressure, the latter pressure lies outside it.

In the upper reversing layer a new feature begins to present itself—the selective effect of radiation pressure near a wave-length of selective absorption. Now it must be supposed that selective absorption is occurring to some extent at all depths throughout the sun; in the far interior the X-ray levels of the atoms will be giving rise to selective absorption. We may, therefore, pause for a moment to enquire how it is that selective radiation pressure only arises on the fringe of the sun. The pressure of radiation at any particular wave-length is proportional to two factors. One is the net outward stream of radiation—the difference between the inward and outward streams; the other is the selective absorption co-efficient in that wave-length—the degree of obstruction offered. But the selective absorption has itself an influence on the net stream. Where the selective obstruction is high, neither an inward nor an outward beam can go very far without being absorbed, and consequently the average distance (from a given atom) from which either an inward or outward beam originates is very small. When the state is one of "local thermodynamic equilibrium," as can be shown to hold, provided the density is not too low, this has the effect of making the inward and outward beams very nearly equal, at a wave-length of strong selective absorption. They originate from places so close to one another both in space and in temperature that they are only slightly unequal. They accordingly very nearly cut one another out. Thus in the product (selective absorption co-efficient) × (net stream) the effect of the large selective absorption co-efficient is neutralised by the small net stream, and we get no appreciable effect of selective radiation pressure; the radiation pressure is the same at a wave-length of selective absorption as at a wave-length where there is no selective absorption.

Let us now see what happens near the boundary of the star, assuming local thermodynamic equilibrium holds up to the boundary. Near the outer confines of the star the inward stream is small, and there is little to neutralise the outward stream. The net stream is almost equal to the outward stream, and a big selective absorption co-efficient will give rise to a big selective radiation pressure.

We can look at the matter from a slightly different angle. At the outside of the star, the big selective absorption co-efficient, combined with such outward stream as exists, gives rise to a big selective radiation pressure, even though the big selective co-efficient has itself cut down the outward stream to a value below the photospheric value. As we go inwards, an inward stream is generated by the back-radiation from the atoms traversed, and this soon nearly balances the outward stream, giving a small net stream in the interior.

At all depths selective absorption cuts down the intensity of each stream. The difference between the interior and the surface is that in the interior the two streams are cut down to nearly the same amount and balance one another, whilst at the surface the outward stream, though cut down, is unbalanced.

The consequence of this is that in the upper reversing layer the gases are pushed outwards by selective radiation pressure, and so the layer is less compressed than the lower reversing layer and photospheric layer beneath. It is not easy to make an exact calculation, but it appears roughly that for calcium atoms the pressure decreases from 10^{-5} atmos. to some very small pressure in a range of about 100 kms. This estimate is not to be pressed—it may perhaps be an under-estimate. But we shall not go far wrong in attributing a thickness of the order of some hundreds of kilometres to the upper reversing layer.

In this region, in a steady state, the atoms are maintained in equilibrium under gravity, the gradient of gas-pressure and selective radiation pressure. As we go outwards, selective radiation pressure steadily increases in importance. The question arises, what happens at the upper boundary of this layer? Actually it cannot have a definite upper boundary, but we will suppose we are endeavouring to trace the pressure upwards by the same principles as govern the equilibrium of the earth's atmosphere, selective radiation pressure only being added. Two possibilities arise: either selective radiation pressure, though increasing, remains steadily less than gravity, or it attains a value greater than gravity. It may do the former for some kinds of atoms, the latter for other kinds of atoms. In the former case nothing particular happens; the atoms of this particular kind simply thin out moderately rapidly. In the latter case we arrive at a contradiction. When radiation pressure exceeds gravity, equilibrium is no longer possible unless the gradient of gas-pressure becomes reversed. As it is difficult to see how the gas-pressure could ever begin to decrease again once it began to increase—and it must ultimately decrease because the sun's atmosphere is finite—we reject this possibility. But if equilibrium is no longer possible, since we have assumed equilibrium all the time, something must be wrong with our assumptions. We could only now keep the atoms in equilibrium if we assumed some kind of hypothetical net which caged the atoms down. Let us suppose that the atoms are in fact kept in equilibrium by a net of this kind, and that then the net is suddenly removed. What will happen?

The atoms, being acted on by a radiation pressure exceeding gravity will be ejected from the layers below. An outer atmosphere will be come into being. But the ejection of atoms will not proceed indefinitely. As this new outer atmosphere becomes thicker and thicker its back radiation will begin to be appreciable: it must be constantly returning to the layers below some fraction of the radiation incident on it. The back radiation will exert a downward pressure, which will go on increasing with increasing thickness of the outer atmosphere until finally its back pressure is equal to the pressure exerted by our hypothetical net. After that no more atoms will be ejected, and the outer atmosphere will be permanent, and will exist in a state of equilibrium.

In the case of the sun we identify this outer atmosphere with the chromosphere, observed at times of total solar eclipse and spectroscopically at other times. About a century ago Canning called a New World into existence to redress the balance of the Old. For millions of years the sun has been calling a new atmosphere into existence to redress the balance of the old.

At a total solar eclipse, when the moon touches the sun's limb internally, a thin crescent of atmosphere is exposed. Its spectrum is a series of bright lines—general absorption at these levels has vanished. By measuring the lengths of the crescents from tip to tip astronomers have estimated the heights to which the atoms of different elements exist. The following table gives samples of the observed heights :—

Heights of Elements in Chromosphere as shown by Lengths of Arcs at different Total Eclipses.

		Lockyer	Mitchell	Davidson and Stratton
		1898	1905	1925
		Kms.	Kms.	Kms.
H	λ 6563 (Hα)	7500	—	8400
	4861 (Hβ)		8000	8400
	4341 (Hγ)		8000	8000
	4101 (Hδ)		8000	7400
	3889 (Hε)		8500	7000
He	λ 4713 ($1^2P - 3^2S$)	—	3900	6000
	5876 ($1^2P - 2^2D$) (D_3)	—	7500	7500
	4471 ($1^2P - 3^2D$)	6500	7500	7400
	4026 ($1^2P - 4^2D$)	4500	6000	4400
	4922 (1 P – 3 D)	—	1000	2500
	4388 (1 P – 4 D)	—	2000	2000
	4144 (1 P – 5 D)	—	1500·	2200
He+	λ 4686 3 D – 4 P)	—	2000	2500
Na	λ 5896, 5890 ($1^2S - 1^2P$) (D_1, D_2)...	—	1200	1000
Ca	λ 4227 (1 S – 1 P)	3500	5000	2500
Ca+	λ 3933, 3968 ($1^2S - 1^2P$)	9500	14000	9200
Sr+	λ 4215, 4077 ($1^2S - 1^2P$)	4500	6000	5200

It will be seen that the thickness in each case is of the order of thousands of kilometres. Roughly speaking, the sequence is: photospheric layer of the order of tens of kilometres, reversing layer of the order of hundreds of kilometres, chromosphere of the order of thousands of kilometres. We may complete the sequence by mentioning that the solar prominences are of the order of at least tens of thousands of kilometres, and that the corona is of the order of hundreds of thousands of kilometres.

The mere size of this thickness shows that we are compelled to introduce some new force other than gravity and gas-pressure. In ordinary gravitational equilibrium, under solar conditions, the pressure would decrease by a factor of 0·07 in 10 km. In 10,000 kms. it would decrease by a factor 10^{-1140}. At this pressure there would be no atoms at all at 10,000 kms., and the pressure would have fallen to 10^{-18} atmos. in 100 kms. from the limb. When selective radiation pressure is introduced the pressure decreases much more slowly.

To see how this arises we must return to our arguments about selective radiation pressure near the boundary. We have still to explain what was wrong with our former assumptions. It will be remembered that we assumed "local thermodynamic equilibrium." This means that we assumed a definite temperature at each point. In strict thermodynamic equilibrium, as in an enclosure at a uniform temperature, the matter is traversed by radiation corresponding to its own temperature. At any point in a star the matter is traversed by radiation arising from places at a variety of temperatures, and strict thermodynamic equilibrium does not occur. But for many purposes the matter behaves as though it had a definite temperature. It can be shown, in fact, that when the density is sufficiently high, that is, when the atoms are sufficiently battered about by collisions with other atoms, the matter behaves according to the laws of Kirchhoff as far as the emission and absorption of radiation is concerned. The ratio of emission to absorption at any point is determined by Kirchhoff's universal function (Planck's function), which contains the temperature as a parameter. When we calculate the laws of transfer of radiation under these conditions using Schwarzschild's equations, we find first the laws of selective radiation pressure mentioned earlier. We find also that energy is continually being transformed to longer and longer wave-lengths as it runs down the temparature gradient, the mechanism being inelastic and hyper-elastic collisions. Hyper-elastic collisions remove energy of shorter wave-lengths, and inelastic collisions introduce radiation or longer wave-lengths.

But as the density decreases collisions become less important. Radiation, instead of being transformed in wave-length, tends to be handed on at the same wave-length. Ultimately, at sufficiently small densities, the whole of the radiation passes unchanged in wave-length. We call this state of affairs "monochromatic radiative equilibrium."

Each portion of matter them emits as much radiation of the wave-length in question as it absorbs. Since there is no gain or loss or radiation of the given wave-length at any point, the net amount of λ-radiation crossing any one level—the difference between the inward and outward streams—is the same as that crossing any other level—for otherwise there would be either an accumulation or a loss of energy between the two levels. The net flux in a given wave-length is therefore constant. Hence if the absorption coefficient is constant, the selective radiation pressure will be constant.

We can now trace the origin of our previous contradiction. First, we erroneously assumed local thermodynamic equilibrium to hold right up to the boundary of the star. This led to the possibility of an uncompensated radiation pressure at the boundary. This, we saw, must cause the formation of an outer atmosphere, a chromosphere. Secondly, we ignored the inward stream of radiation arising from this outer atmosphere; we calculated the selective radiation pressure at the boundary of the layer in local thermodynamic equilibrium as though there were no inward stream there.

When we now take account of the inward stream from the outer atmosphere we see that all contradictions are removed. The boundary of the region in local thermodynamic equilibrium is not the boundary of the star as a whole. Outside this there will exist in general, for each particular kind of atom, a region not in local thermodynamic equilibrium but in monochromatic radiative equilibrium. This acts as a cushion, gently pressing on the region beneath and keeping it in equilibrium, to a slight extent by its weight but principally by its back radiation.

In this region, the chromosphere, we can now see that the density is likely to fall off very slowly. A decrease of density upwards means a decrease of pressure, and hence a pressure gradient. This balances part of the weight. But we have seen that the creation of the outer atmosphere will go on until its weight is just sufficient to balance the otherwise uncompensated radiation pressure from the region beneath. In this equilibrium there is no part left for pressure gradients to play, and they will be practically non-existent. In other words, the density will be practically constant.

Actually this overstates the case. The density must, in fact, decrease slightly outwards. For it to be accurately constant, the radiation pressure would have to be accurately constant. We have seen that the net flux is accurately constant. But the radiation pressure is proportional to the net flux and the absorption co-efficient, and the latter is not accurately constant. It must be smaller at lower levels, because owing to the increased density of radiation there (forward plus backward stream) more of the atoms at any instant will be excited and so not in a condition to absorb; thus the average absorption co-efficient will be smaller. The consequence is that, at least at the lower levels, there must be some pressure gradient to take care of the uncompensated portions of gravity, and so there will also be a density gradient.

Theoretically the density should fall off as the inverse square of the height measured from a certain datum level.

A further point is that though we have demonstrated the possibility of the existence of an extended low-pressure outer atmosphere with small density-gradient, we have not proved that this is the only type of outer atmosphere. There are a whole range of other possibilities. If, after expelling enough atoms to form an outer atmosphere, the lower regions exude for some reason more atoms, radiation pressure will further diminish. There will be too many atoms to be fully supported by radiation pressure, and the outer atmosphere will partially collapse on to the lower regions. It will then be strongly condensed towards its base. Nevertheless, an all-but-fully supported outer atmosphere is what is observed in the case of ionised calcium atoms on the sun. The spectral observations made by Col. Stratton and Mr. Davidson at the eclipse of 1926 (Sumatra) have been analysed by Mr. P. A. Taylor. He first extended the theory so as to allow for the curvature of the sun, and then compared the observed outward decrease of intensity of the flash spectrum with the calculated decrease. It appeared that all but about one ten-thousandth of the weight of the calcium chromosphere was supported by radiation pressure. Why this is so we cannot exactly say. One suggestion is that it appears possible that a strongly condensed "partially supported" chromosphere would tend to re-introduce local thermodynamic equilibrium, in which case atoms would be once again expelled and a fresh chromosphere would be grown. This is a mere conjecture, and such calculations as I have been able to make do not confirm it. We must await further investigations.

Each species of atom occurring in the chromosphere should produce its own radiation gradient, independent of the others, and the complete chromosphere arises from the co-existence of the separate outer atmosphere for the separate species of atom.

We have arranged our argument as though the region of local thermodynamic equilibrium ends abruptly, and is superposed by a region in monochromatic radiative equilibrium. Actually the two must shade into one another. The precise pressure at which the one set of conditions gives place to the other depends on the effective cross-section of the atom for collisions that are capable of exciting it, and if this cross-section is of the order of magnitude of the cross-section given by the dynamical theory of gases then the transition pressure where an intermediate state holds is of the order of 10^{-3} atmos. If this is to be trusted, it would mean that all lines in the reversing layer are produced under conditions where on the whole the state is one of monochromatic radiative equilibrium rather than one of local thermodynamic equilibrium.

As a corollary it would follow that all lines except those due to very heavy atoms should appear in the chromosphere. Roughly speaking, this is what is observed.

The more nearly an outer atmosphere, in a particular kind of atom, is in monochromatic radiative equilibrium, the more nearly the ratio of the residual intensity in the line-centre at the limb to that at the centre of the disc should approach the value 2/5. It will be a matter of interest to see to what extent this holds—investigations are now in progress at a number of different observatories. Schwarzschild pointed out long ago that on local thermodynamic equilibrium all absorption lines should vanish in the continuous background at the limb, and inferred that since they do not do so there must be a measure of monochromatic radiative equilibrium. Our general theory bears this out. For all atoms, local thermodynamic equilibrium must give place to monochromatic radiative equilibrium at low densities, and the more completely this holds the more completely should the limb-centre ratio approach 2/5. Schwarzschild found that the ratio was, in fact, about 2/5 for the H and K lines of calcium, for which lines the existence of a high-level chromosphere makes it very probable that monochromatic radiative equilibrium holds strictly.

It is perhaps scarcely necessary to add that in the atmospheric structure we have sketched we have highly idealised the problem at a number of points. We have simply attempted to construct a model of the solar atmosphere useful for the points under discussion. It will not necessarily be useful for other points. Even as it stands it is not a complete model. For one thing, we have tacitly dealt only with electronic transitions as the origin of lines—we have ignored the effects of selective scattering as the cause of line-widening, as suggested by Stewart and brought into prominence by the work of Unsöld.

Again, we have argued as though the boundary between monochromatic radiative equilibrium and local thermodynamic equilibrium coincided with the boundary between the low-pressure chromosphere and the higher pressure reversing layer. Actually we have found that monochromatic radiative equilibrium is coming into being at about 10^{-3} atmos., whilst, for example, the partial pressure of calcium at the base of the chromosphere can scarcely exceed 10^{-12} atmos. This involves no intrinsic self-inconsistency. It is merely that we have not dealt adequately with the few hundred kilometres in which this transition occurs. Somehow or other the sun knows how to arrange its layers in this region in accordance with the laws of physics, and in so doing it propounds an attractive puzzle for the mathematician and the solar physicist.

[E. A. M.]

WEEKLY EVENING MEETING,

Friday, March 16, 1928.

Sir Robert Robertson, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Edmund T. Whittaker, LL.D. Sc.D. F.R.S.,
Professor of Mathematics, University of Edinburgh.

The Quantum and Relativity Theories of Light.

The classical theory of light, which was created by Fresnel in 1816–23, and was transformed into an electromagnetic theory by Maxwell in 1861–62, was believed, at the end of the nineteenth century, to be capable of accounting for all optical phenomena. Since then, however, a number of phenomena have been observed which appear to be irreconcilable with it, at any rate in the form in which it is usually presented. These phenomena relate to the transfer of energy between radiation on the one hand, and atoms of matter on the other. For example, when X-rays fall on a metallic plate, electrons are set free and ejected from the plate with definite velocities; in this "photo-electric" effect, the velocity with which each electron is endowed at its liberation is altogether independent of the intensity of the X-rays, and depends only on their frequency. When the X-rays are feeble in intensity, we are bound to suppose that, in order to acquire this velocity, the electron has somehow been enabled to collect energy from a large area of the cross-section of the incident beam. But it was shown by the late Lord Rayleigh that on the principles of the classical theory, the area of a wave-front of light of wave-length λ which can be tapped and have its energy extracted by a small resonator is something comparable with (λ^2/π); and this is altogether inadequate to explain the experimental results. We must therefore conclude that the classical theory fails to account for the phenomena.

As an alternative hypothesis it was suggested by Einstein in 1905 that light may be concentrated in very small parcels or "quanta," which retain their energy undiminished as they travel out into space, without the unlimited spreading which takes place with ordinary diverging waves: the atom would then be supposed to capture a whole quantum at once and absorb its energy, which is of amount $h\nu$, where h is Planck's constant, and ν is the frequency of the light. This hypothesis accounts satisfactorily for photo-electric phenomena, and also for the charge of wave-length of X-rays due to scattering (the Compton effect), and the production of recoil electrons.

The hypothesis that light travels in small compact parcels may, however, be reconciled with the classical electromagnetic theory, if we assume that the parcels contain electric or magnetic charge which is travelling with the velocity of light; in fact, the field surrounding electric or magnetic charge travelling with the velocity of light has all the ordinary classical properties of light, except that of spreading; the energy of the field remains permanently linked up with the moving charge, so that an atom which captures the moving charge thereby captures all the luminous energy associated with it.

This way out of the difficulty was, until quite recently, open to the objection that, while it accounted satisfactorily for the propagation of light through free space in non-spreading quanta, it did not help in any way to clear up the apparent contradictions between the classical theory and Bohr's theory of the emission of spectra. According to the classical theory, we should expect that radiation of frequency ν would be produced by an electron, or electronic system, vibrating or performing an orbital revolution, with the same frequency ν. But in Bohr's theory, which has been remarkably successful in explaining spectra, monochromatic radiation is produced when an electron within an atom falls from an orbit, in which its energy is (say) H_1, to another orbit in which its energy is (say) H_2, the frequency ν of the radiation being determined by the relation

$$h\nu = H_1 - H_2;$$

so that in Bohr's theory, the frequency of the emitted radiation is quite diffierent from the frequency of the oscillations or revolutions taking place in the atom. This discrepancy has attracted a tremendous amount of attention during the last fifteen years, and has led some physicists to speak somewhat hastily about the "failure" of the classical theory. Within the last two years however, this particular trouble has been removed, and a complete reconciliation with the principles of the classical theory has been effected, by means of Schrödinger's wave-mechanics, the argument being as follows:

In Schrödinger's theory the phenomena are described in terms of a "wave-function" ψ, which satisfies a certain partial differential equation—just as the vibrations of a violin string may be worked out mathematically by means of a function which represents the displacement of a particular point of the string at a particular instant of time, and which satisfies a partial differential equation. In the case of the violin string we know that there are certain special solutions of the partial differential equation which represent particularly simple states of vibration of the string—which represent, in fact, the pure fundamental note and its various overtones; similarly, in the case of Schrödinger's wave-function, there are certain particular solutions of the partial differential equation for ψ, which correspond to what in Bohr's theory were pictured as the "stationary states" of the atom. There is, however, this great difference between Bohr's original theory and Schrödinger's theory, that in Bohr's theory a stationary state meant a particular kind of orbital motion, so that an atom could be in only one stationary state at one time; whereas, in Schrödinger's theory the various stationary states can be superposed (since they are merely different particular solutions of a linear partial differential equation), just as overtones can be superposed on the fundamental tone of a violin string; and this makes all the difference when it comes to reconciling our ideas with those of the classical theory.

Suppose then that we consider an atom in one of Schrödinger's stationary states, so that the wave-function ψ involves the time t through a factor $e^{2\pi i\nu_1 t}$. The electric moment of the atom is the integral of $\psi\bar{\psi}$, when $\bar{\psi}$ is the complex quantity conjugate to ψ, this integral being integrated in a certain way over the atom. Now since ψ contains the factor $e^{2\pi i\nu_1 t}$, it follows that $\bar{\psi}$ will contain the factor $e^{-2\pi i\nu_1 t}$, and therefore in the product $\psi\bar{\psi}$ these factors will destroy each other: that is to say, when the atom is in one of Schrödinger's stationary states, its electric moment does not vary with the time, and therefore, *according to the classical theory*, it will not emit radiation.

Suppose next, however, that the atom is not in a pure stationary state, but is in a state which is represented by the superposition of two stationary states, for which the wave-function has the time-factors $e^{2\pi i\nu_1 t}$ and $e^{2\pi i\nu_2 t}$ respectively, so that

$$\psi = A e^{2\pi i\nu_1 t} + B e^{2\pi i\nu_2 t},$$

where A and B do not involve the time. Then we have

$$\psi\bar{\psi} = (A e^{2\pi i\nu_1 t} + B e^{2\pi i\nu_2 t})(\bar{A} e^{-2\pi i\nu_1 t} + \bar{B} e^{-2\pi i\nu_2 t})$$

$$= A\bar{A} + B\bar{B} + A\bar{B} e^{2\pi i(\nu_1 - \nu_2)t} + B\bar{A} e^{-2\pi i(\nu_1 - \nu_2)t},$$

and hence the electric moment of the atom will be periodic, with frequency $(\nu_1 - \nu_2)$; and consequently the atom, *according to the classical theory*, will emit radiation of frequency $(\nu_1 - \nu_2)$. This radiation will continue until the consequent exhaustion of energy has again reduced the atom to a single pure stationary state.

If E_1 and E_2 are the energies associated with the two stationary states, then $E_1 = \lambda\nu_1$ and $E_2 = \lambda\nu_2$, so the radiation is of frequency $\frac{1}{\lambda}(E_1 - E_2)$, just as in Bohr's theory; but this result has now been obtained by what are essentially classical methods—by the old classical theory of the normal solutions of partial differential equations—and without doing any violence to the electromagnetic theory of light. The classical theory, in short, has swallowed up the quantum theory.

I do not wish to be understood as expressing the opinion that the present state of Schrödinger's wave-mechanics is definitive and final. It seems to me that certain modifications will have to be made in it in the near future, in particular the replacing of Schrödinger's single scalar wave-function ψ by a vector or tensor of some kind. But it is a very great advance on anything that has preceded it; and the remarkable experiments of Davisson and Germer in America, and of G. P. Thomson in this country, on the scattering of cathode rays have substantiated beyond doubt its leading idea, that there is something of an undulatory character associated with a beam of electrons.

It seems probable that before long the two most interesting branches of modern theoretical physics—wave-mechanics and general relativity—will make approaches towards each other, and tend to become fused in a more comprehensive doctrine. Meanwhile, from the side of relativity, some progress has been made in studying the influence of gravitation on electromagnetic phenomena—an influence which may be of importance in the nucleus of an atom. It is now recognised that a gravitational field is accompanied by, or (perhaps we should say) consists in, a "curvature" or "distortion" of space, which, if the gravitational field is sufficiently intense, can have a remarkable influence on any electromagnetic phenomena which may be taking place there. This influence resembles in a general way that which would be observed if the dielectric constant and magnetic permeability varied from point to point of space, giving rise of course to an apparent variation of the refractive index. The only case in which effects due to this cause have been actually observed is the famous case of the deflection of the rays of light from stars in passing close to the gravitating mass of the sun, a phenomenon which was predicted by Einstein and measured on the plates taken at the eclipse of May 1919. The solar deflection is very small; but it is possible that in the intense gravitational fields inside the nucleus of an atom, electromagnetic phenomena may be more powerfully modified. Certainly the most remarkable happenings are possible in theory; thus, a ray of light can describe a circular path, returning into itself; and in other ways luminous energy may, by the mere distortion of space, be confined perpetually to a small definite region of space, just as if it were confined in a box with reflecting sides. The capture of a ray of light by a gravitating centre may be studied mathematically; and amongst other results it is found that radiation is emitted by an electron which is permanently at rest, provided it is placed in an alternating gravitational field. The knowledge that a motionless electron may radiate, while (as a natural consequence) an accelerated electron does not necessarily radiate, in a gravitational field, may prove useful in accounting for the behaviour of electrons in atoms. But whether it will or not, time alone can show.

[E. T. W.]

WEEKLY EVENING MEETING,

Friday, March 23, 1928.

SIR ARTHUR KEITH, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

ERNST COHEN, D.Sc. For. Mem. R.S. Hon. M.R.I.,
Director, Van t'Hoff Laboratory, University of Utrecht, Holland.

The Physical Chemist in Search of Purity in an Impure World.

[The subject matter of the Discourse is published in the Journal of the Society of Chemical Industry.]

GENERAL MONTHLY MEETING,

Monday, April 2, 1928.

SIR ARTHUR KEITH, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Daniel Hall, K.C.B. F.R.S.
Mrs. Harriet Oldfield Hamilton, M.Sc.
Basil Walter Leefe,
Sir William Mitchell, K.C.M.G.
William Percival Paddison,
Evan Parry,

were elected Members.

The Secretary reported the death of Sir David Ferrier on March 19, and it was unanimously

RESOLVED, That the Members of the Royal Institution of Great Britain desire to place on record their sense of the great loss sustained by the Institution and the Science of Medicine by the death of Sir David Ferrier, Emeritus Professor of Neuropathology, King's College, London, M.D.(Edin.) LL.D.(Aber.) Sc.D.(Camb.) F.R.S. F.R.C.P. M.R.I.

Sir David Ferrier was a pioneer in the science of Neurology, and the author of the Functions of the Brain (1876), and Cerebral Localisation (1878). He mapped out the motor areas of the Brain, and localised the perceptive centres.

As a Member of the Royal Institution Sir David Ferrier displayed a deep interest in its welfare and the promotion of its objects.

The Members desire to express their sincere sympathy with Lady Ferrier and the family in their bereavement.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., etc., was nominated for Election as Honorary Professor of Natural Philosophy at the next General Meeting on Monday, May 7, 1928, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. Pres.R.S., etc., was nominated for Election as Professor of Natural Philosophy at the next General Meeting on Monday, May 7, 1928, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

Cornish, Vaughan, Esq., D.Sc. (the Author)—Harmonies of Scenery. 8vo. 1928.

Longmans Green & Co., Ltd.—The House of Longmans, 1724–1924. By H. Cox and J. E. Chandler. 8vo. 1925.

Marconi, Senatore G., M.R.I. (the Author)—Radio Communication. 8vo. 1928.

Parker, W. Rushton, Esq., M D. M.R.I.—On a Specimen of *Elephas Antiquus* from Upnor. By C. W. Andrews. 4to. 1928.

Shirley Institute, The Secretary—Memoirs, Vol. VI. 1927. 8vo. 1928.

WEEKLY EVENING MEETING,

Friday, April 20, 1928.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

COLONEL SIR HENRY G. LYONS, D.Sc. F.R.S. M.R.I.,
Director and Secretary of the Science Museum.

Heirlooms of Industry in the Science Museum.

THE idea of a general museum of Science is only seventy-five years old, and was due to the Prince Consort, who, after the Great Exhibition of 1851, urged the formation of an institution which would extend the influence of Science and Art on productive industry. From this proposal came the South Kensington Museum, the dual activities of which are now represented by the Science Museum and by the Victoria and Albert Museum respectively. Scientific instruments, pieces of apparatus, mechanical devices and such like had been preserved in many places, but a museum designed to illustrate the influence of science on technical development did not exist; even the Museum of the Conservatoire des Arts et Metiers in Paris, which dates from the end of the eighteenth century, was primarily the teaching collection of the Conservatoire. Recently technical museums having similar aims have been established at Munich in 1903, and at Vienna in 1919.

While no one will dispute the utility of technical museums, it must be admitted that they fall far short of art museums in the attractiveness of the objects which they contain, and this fundamental difference affects every stage of museum arrangement. For a technical museum to be interesting something of their history and their purpose, the part which they have played in the age-long development of the branch of industry to which they belong, must be known to the visitor, and to this end carefully edited labels are essential; important objects may be so displayed that the internal working parts can be seen, and their purpose understood; coloured diagrams and specially arranged illumination may be employed; models, etc., may be shown in motion; others may be so arranged that they can be set in motion by the visitor.

The dominating principle in a technical museum must be development. Everything, whether it be an industry, or a group of related objects, or a type of tool, is shown so as to emphasize the successive stages of development which have been traversed from early crude forms which sufficed in the days of hand labour through various grades of slow improvement to the rapid advances of modern times so effectively aided by steam and electricity. In some branches of human activity it is instructive to show a few examples from the times of the earliest civilisation, and from the handiwork of primitive races who exist to-day. In this way the story of time measurement, of various hand tools, and of land and water transport can be illustrated far more effectively and attractively than if restricted to the last few centuries.

Aeronautics furnish a convenient example of rapid development, for air transport has grown from its first experimental stages to an important branch of world communication within the lifetime of many of those present this evening. The series exhibited in the Science Museum begins with the first power-driven model aeroplane which John Stringfellow constructed in 1848, and which achieved a free flight of forty yards. Experiments in "gliding flight" were of great importance in providing information on many points in aerodynamics, and Otto Lilienthal was its greatest exponent. One of his gliding machines of 1896 is exhibited. Professor Samuel P. Langley, of the Smithsonian Institution at Washington, designed and constructed a man-carrying tandem monoplane, of which a model is on exhibition, but it failed to make a successful flight when tried in 1903.

Inspired by the experiments of Lilienthal in gliding flight, the brothers Wilbur and Orville Wright, of Dayton, Ohio, carried out a large number of gliding flights during several years, and the success which they achieved encouraged them to construct their first power-driven aeroplane, and they made their first successful flights on December 17, 1903, the longest being one of fifty-nine seconds, when the distance covered was 852 feet. The aeroplane with which they made their flight has been lent to the Museum by Mr. Orville Wright. Mr. A. V. Roe's light tractor triplane, which was built and flown by him in 1909, is remarkable for low power of the engine, 9 h.p. twin-cylinder J.A.P. engine. Another interesting type is the Antoinette model, of French design, which was flown across the Channel by H. Latham in July 1909. Mr. S. Cody's machine, which he used in the military manœuvres of 1912, is preserved in the Museum. This series is fittingly concluded by the Vickers Vimy Rolls Royce aeroplane on which Sir John Alcock and Sir Arthur Whitten Brown made the first direct trans-Atlantic flight in June 1919. The extraordinarily rapid development of this form of transport is strikingly demonstrated by the fortunate juxtaposition of the Wright Brothers' aeroplane, which flew 284 yards in 59 seconds in December 1903, and the Vickers Vimy aeroplane, which covered 1890 miles in 15 hours 57 minutes in June 1919, only 15½ years later.

Road transport also provides a series in which the transition from early types to modern practice is strikingly shown—that of the bicycle. Starting from the hobby horse of 1818, the first practical form of two-wheeled machine was the early bicycle invented by Kirkpatrick MacMillan, a blacksmith, of Dumfriesshire, in which he anticipated the rear-driving safety bicycle of forty years later. In Michaux's bicycle of 1865, one of the so-called "boneshakers," the Starley Spiderwheel Bicycle, and Lawson's bicyclette of 1879, bring us to the safety bicycles of to-day.

Just as in an industry, so with each tool or machine, the successive stages by which the early form has advanced through others of increased efficiency, or of greater convenience, bear witness to the inventiveness of successive generations, and, wherever possible, this is illustrated. Miners' lamps are represented by a long series, including every type of importance from Sir Humphry Davy's to the latest pattern of the modern lamp. In the same way a most instructive series of radio-valves, beginning with original valves of Professor Fleming, continuing through many modern types.

In Machine Tools, the primitive form of lathe, in which the work is rotated by means of the forward and backward movement of a bow held in the right hand, replaced later by the spring beam, or pole drive, and this type is still used by the chair-makers of High Wycombe. About 1800 Henry Maudslay combined the slide rest and the use of a lead screw with change wheels for the production of screw threads, and the first workshop tool in which Maudslay embodied his invention is preserved in the Museum. A further advance is shown by Roberts' back-geared slide lathe of 1817.

The early history of the hammer is illustrated among the hand tools, but mechanical devices were early adopted. A lift hammer of 1556 is shown by a model, and lift and tilt hammers were still in use at the beginning of the nineteenth century; two hammers which were erected in Portsmouth Dockyard in 1804 by Simon Goodrich are shown by a model. In 1839 James Nasmyth invented his steam hammer, which is represented by a working model, and other models show the various improvements which were introduced at later dates. The great power presses of to-day are too large and heavy to be shown, but illuminated photographic transparencies are utilised to show the form and character of those used in modern practice.

The early forms of any machine or tool are now of the greatest interest to us as we compare them with later developments, but only in few cases have they been preserved. Fortunately a considerable number of such early types have been acquired from time to time for the nation, among which are:—

Arkwright's original Drawing Frame of 1780, in which the drawing of cotton is effected by successive pairs of rollers revolving at increased rates of speed.

Two of James Watt's double acting rotative beam engines of 1788 and 1797. Among locomotives the original "Puffing Billy" of 1813, and the "Rocket" of 1829.

In marine engineering, among many notable landmarks, are the engine of Bell's "Comet"; also the T.S. "Turbinia," and the turbine engines with which she steamed thirty-five knots per hour at Spithead.

The collection of ship models contains a number of early and important contemporary models which furnish details of hull construction and of rigging which are not otherwise readily obtainable; they include Christopher Columbus's ship, the "Santa Maria"; the "Prince," which was built at Chatham by W. Pett in 1669; also a contemporary model of a 64-gun man-o'-war of 1805, which has supplied much information for the recent re-rigging of the "Victory" at Portsmouth. Another very instructive model is that of a yacht of 1660, built in the style of the Dutch vessels of that period.

The wholesale disappearance of the earliest forms of technical instruments, to which reference has already been made, has often deprived us of all but a single specimen of its type. An accurate copy is then the only way of representing it in a collection, and in recent years a number of reproductions of this kind have been acquired by the Museum. They include an astronomical instrument of Egypt ("Merkhet"); a shadow clock of the same country, both dating from about the 8th or 10th century B.C.; the Groma, as used by the Roman land surveyors, and also one of their 10-foot rods; two telescopes made by Galileo, which are now preserved at Florence; Newton's reflecting telescope, made by himself, and now in the possession of the Royal Society; and a copy of a 14th century raingauge from Korea.

It only occasionally happens that the life-work of one of the great inventors of the past can be suitably shown in a museum, but in the case of James Watt, the attic workshop in which he worked during the last twenty years of his life has been reproduced, and in it are placed all the machines, tools, and other contents of the original room at Heathfield, near Birmingham.

[H. G. L.]

WEEKLY EVENING MEETING,

Friday, April 27, 1928.

William Stone, M.A. F.L.S. F.C.S., Manager,
in the Chair.

Arthur M. Tyndall, D.Sc. F.Inst.P., Professor of Physics,
University of Bristol.

Carriers of Electricity in the Atmosphere.

The nature of ions in air has been a subject of study for more than thirty years, but our information is still incomplete, and a variety of phenomena still require elucidation.

A certain amount of information on the nature of ions in the lower atmosphere may be gained from a study of their motion in an electric field. Except in special cases, which rarely arise at ordinary pressures, the motion of an ion through the air is analogous to the motion of a sphere falling through a viscous liquid. This motion is one of uniform velocity, the value of which depends upon the radius of the sphere, the force acting upon it and the viscosity of the liquid.

For a given force and medium the larger the sphere the slower it moves. This may readily be demonstrated in a syrupy liquid by dropping into it two balls, one of lead and the other of aluminium, their relative sizes having been selected so that they have equal weights in the liquid.

Similarly ions in air possessing the same electric charge, but having different sizes, will move in an electric field at different rates, the larger one travelling slower. There are theoretical grounds for supposing that this factor of size, though not the only one, is of great importance in determining the motion of the ion.

Some interesting effects may be obtained by adding small quantities of an organic vapour to the air. Let us take, for instance, the series of normal alcohols of chemical composition $CH_3\ (CH_2)_n\ OH$ where $_n$ may be zero or an integer. The molecules of these substances are known to be rod-like in shape, the length of the rod increasing with increase in the carbon content, i.e. with the value of $_n$. They are also known to be polar, a property presumably mainly due to the OH group made up of a positive hydrogen nucleus and a negative atom of oxygen. For the purposes of a rough static model these molecules may therefore be thought of as having an active head and a more or less inert tail, and they will be attracted to a negative ion with their heads inwards towards the ion and their tails spread out radially.

The effective size of the ion will thereby be increased by an amount which depends on the length of the molecules of the particular alcohol added.

Experiments by the lecturer and L. R. Phillips have shown that for a given vapour pressure of alcohol the reduction in mobility of an ion increases rapidly with increase in length of alcohol chain. Thus the highest alcohol (Amyl) used in this work, though present as only one part in 300 of air, reduced the mobility of the ordinary negative air ion to about 40 per cent. of its normal value.

The effect on positive ions is nothing like so marked, though it is observable. This may be attributed to a weaker bond between the dipole and a positive ion, because the positive end of the dipole cannot approach it so closely.

By adding water vapour as well as alcohol vapour to the air there is now a competition for places at the ion surface, so that short water molecules replace some of the longer alcohol molecules. We should on this view expect a rise in mobility of the ion, and this is observed.

If the molecules are made non-polar by removing the OH group and making them symmetrical in structure, the tendency to cluster on the ion should be almost entirely removed. This has also been confirmed by experiment, since it has been found that the hydrocarbon decane with ten carbon atoms, and therefore of length roughly twice that of amyl alcohol molecules, has practically no effect on the motion of the ion in air.

New methods for measuring mobility have been devised with the special purpose of deciding whether all the ions move with the same speed. Considerable resolving power has been achieved. The negative ions appear to be of a single kind in air containing appreciable quantities of vapour. The positive ions over the same range, and the negative ions in the presence of small traces of vapour, appear to be more complex. Certain features present themselves which are still the subject of investigation.

A lecture demonstration cf the loading effect of alcohol may be conveniently made by applying a small voltage to an ionization chamber, so that the ionization current is well below saturation. By blowing alcohol vapour into the chamber the current is reduced to about a half, due to the reduction in mobility of the ions conveying it. The ionization current may be amplified by a valve method so that it is recorded on a galvanometer.

In the ordinary atmosphere complicating factors are introduced by the presence of particles of dust, smoke, mist, and other nuclei. A number of small ions will attach themselves to these, and will then move so slowly that their contribution to an ionization current is practically negligible. In the demonstration referred to above this may be readily shown by blowing tobacco smoke into the chamber, when the ionization current to all intents ceases. In addition to the production of large ions by the union of small ions with nuclei, large ions may also be produced by splashing and breaking up of water-drops, by frictional effects in dust storms, and so forth. The presence of these large ions has marked local effects on the potential gradient at the earth's surface and the value of the air earth-current at a given place. Certain fluctuations in these values have been correlated with variations in the number of nuclei present.

When ions of a given sign are dragged through a gas by an electric field they set the air in motion. With the relatively intense ionization current from an electrified point, this gives rise to the well-known phenomenon of the electric wind. One of the earliest methods of measuring the mobility of ions was based upon a study of this phenomenon.

Again, if a discharge of this type takes place in a smoky atmosphere the electric wind assists in carrying the charged smoke particles towards the surrounding surfaces, where the particles stick on impact. In other words, it acts as a smoke precipitator.

To sum up, it may therefore be said that the subject of atmospheric ions has a bearing on at least two important problems at the present time. Firstly, there is the problem of the mode and mechanism of attachment of molecules and ions, linking up with allied problems in the structure of bodies in general. And, secondly, there is the wide field of meteorology and the problem of atmospheric electricity in particular.

[A. M. T.]

ANNUAL MEETING,

Tuesday, May 1, 1928.

SIR ARTHUR KEITH, M.D. LL.D. F.R.S. F.R.C.S., Treasurer and Vice-President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1927, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Seventy-five new Members were elected in 1927.

Sixty-three Lectures and Nineteen Friday Evening Discourses were delivered in 1927.

The Books and Pamphlets presented in 1927 amounted to 181 volumes, making, with 581 volumes (including Periodicals bound) purchased by the Managers, a total of 762 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—**The Duke of Northumberland, K.G. C.B.E. M.V.O.**

TREASURER—**Sir Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S.**

SECRETARY—**Sir Robert Robertson, K.B.E. M.A. F.R.S.**

MANAGERS.

The Rt. Hon. Lord Blanesburgh, G.B.E. P.C. LL.D.
Sir James Crichton-Browne, M.D. LL.D. F.R.S.
J. Mitchell Bruce, C.V.O. M.D. LL.D.
Alfred Carpmael, B.A.
Frederick G. Donnan, C.B.E. D.Sc. F.R.S.
Sir James Dundas-Grant, K.B.E. M.D. F.R.C.S.
Viscount Falmouth, L.C.C.
Sir Robert Hadfield, Bart., D.Sc. F.R.S. M.Inst.C.E.
John S. Highfield, M.I.E.E.
W. E. Lawson Johnston.
Col. Sir Henry Lyons, D.Sc. F.R.S.
Charles H. Merz, M.I.E.E.
S. W. A. Noble, M.Inst.C.E.
Sir Richard Paget, Bart.
George C. Simpson, C.B. C.B.E. D.Sc. F.R.S.

VISITORS.

Edward N. da C. Andrade, D.Sc.
Alexander Glegg, J.P.
Commander A. C. Goolden, O.B.E.
W. Vaux Graham, M.Inst.C.E.
Kenneth R. Hay, O.B.E. M.A. M.B.
Sir Lawrence Jones, Bart., J.P. M.A.
Vincent Warren Low, C.B. M.D. F.R.C.S.
William Macnab, C.B.E. F.I.C.
Wyndham A. Milligan, M.D. F.R.C.S.
Emile S. Mond.
Clifford C. Paterson, M.Inst.C.E. M.I.E.E.
Edwin H. Rayner, Sc.D. M.I.E.E.
Hugh Munro Ross, B.A.
Sidney Skinner, M.A. F.Inst.P.
William J. Tennant, M.I.Mech.E.

WEEKLY EVENING MEETING,

Friday, May 4, 1928.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Manager and Vice-President, in the Chair.

THE RIGHT HON. REGINALD McKENNA, P.C.

Credit and Currency.

[ABSTRACT.]

DEFINING money as what the public have to spend, that is to say, currency and bank deposits, Mr. McKenna pointed out that the total of money varied with three factors; first, the amount of currency required in circulation by the public; secondly, the ratio of cash held by the banks, called bank cash, to their deposit liabilities; and, thirdly, the total amount of bank cash. Public demands for currency varied only with seasonal movements and changes in the price level, and for short-term periods might be disregarded as a cause of variation in the quantity of money. Next, the Clearing Banks generally maintained a fixed proportion between their cash reserves, or bank cash, and their deposit liabilities. There remained the third cause of variation in the quantity of money, and as regards this Mr. McKenna showed with the aid of lantern slides that the supply of bank cash was controlled by the action of the Bank of England in buying or selling gold, securities, and other assets, and in extending or calling in loans. Thus it was clear that the quantity of spendable money in the country was determined by Bank of England policy. That policy was primarily based on gold movements, but the effect of such movements in increasing or diminishing the quantity of money could always be counteracted by the sale or purchase of securities. This power in the Bank of England led up to the questions whether it would be sound policy to reconsider its constitution with a view to giving it greater freedom in the exercise of its control over the quantity of money, and whether trade and employment considerations should have more weight than they have had hitherto in guiding the policy of the Bank of England. "These questions," Mr. McKenna concluded, "have a direct bearing upon national prosperity, and are more worth public enquiry than some of the subjects which at one time or other have engaged the attention of Royal Commissions."

[R. McK.]

GENERAL MONTHLY MEETING,

Monday, May 7, 1928.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Vice-President, in the Chair.

The Chairman announced that, in terms of the Bye-Laws, Chap. V, Art. 2, the President, His Grace the Duke of Northumberland, had nominated the following Managers as Vice-Presidents for the ensuing year:—

The Right Hon. Lord Blanesburgh, G.B.E. P.C. LL.D.
Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.
J. Mitchell Bruce, C.V.O. M.D. LL.D.
Sir James Dundas-Grant, K.B.E. M.D. F.R.C.S.
Colonel Sir Henry Lyons, D.Sc. F.R.S.
Charles H. Merz, M.I.E.E.
Sir Arthur Keith, M.D. LL.D. F.R.S., Treasurer.
Sir Robert Robertson, K.B.E. M.A. F.R S., Secretary.

Miss Olga Ingham,
Cuthbert Johnson,
Mrs. R. W. Paul

were elected Members.

Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., etc., was Elected Honorary Professor of Natural Philosophy, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

Sir Ernest Rutherford, O.M. LL.D. D.Sc. Pres.R.S., etc., was Elected Professor of Natural Philosophy, in conformity with the Bye-Laws, Chap. XIX. Art. 2.

The Secretary announced the death of Professor Theodor Curtius on February 9, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss the Royal Institution has sustained by the death of Theodor Curtius, Ph.D., Emeritus Professor of Chemistry in the University of Heidelburg, Honorary Member of the Royal Institution.

Professor Curtius carried out many important researches in Chemistry, and was the discoverer of Hydrazine and the Azides. He was the first to prepare Lead Azide, which has been largely used as a substitute for fulminate of mercury as a detonator. He contributed numerous memoirs to learned societies and to scientific periodicals.

On behalf of the Members, the Managers desire to convey their most sincere sympathy with the family in their bereavement.

The following PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

Aberdeen University (*The Librarian*) — Catalogue of the Publications of Scottish Historical and Kindred Clubs and Societies, 1780–1908. By C. Sanford Terry. 8vo. 1909.

Dawlings, R. Maurice N., Esq., M.R.I. — Pottery and Porcelain. By E. Hannover. 3 vols. 8vo. 1924.

Early Ceramic Wares of China. By A. L. Hetherington. Abridged Edition. 8vo. 1924.

Safety in Mines Research Board—Publications, Vol. II. 1926. 8vo. 1927.

WEEKLY EVENING MEETING,

Friday, May 11, 1928.

SIR JAMES DUNDAS-GRANT, K.B.E. M.D. F.R.C.S.,
Vice-President, in the Chair.

MISS DORIS L. MACKINNON, D.Sc., Professor of Zoology,
University of London, King's College.

Life's Unsuspected Partnerships.

SYMBIOSIS is the word used by biologists to describe the state of affairs in which two or more different kinds of organisms are closely, and in some cases inseparably, associated for the greater part of their lives in a partnership from which both, in some degree, probably draw benefit. Within the last few years many unsuspected interdependences have been revealed, and a vast field has been opened up for further research.

It has recently been claimed by Pierantoni and other workers that the luminescence of surface-living cuttle-fishes, pelagic tunicates, and certain reef-inhabiting fishes is produced by bacteria that are in constant symbiosis with them. Saprophytic light-giving bacteria are abundant in the sea, and are inevitably swallowed by feeding animals, in the dead bodies of which they multiply exceedingly, and, still glowing, produce the disconcerting phenomenon of phosphorescence that may be noticed, for example, in rotting fish.

Among the little sand-hoppers of the genus *Talitrus*, which are normally not luminescent, one is occasionally found glowing with a mysterious inward light. Such individuals are always diseased, and if their infected blood be injected into the bodies of other like crustaceans, these also begin to glow and soon die. It would look therefore as though, for some animals, the incursion of luminescent bacteria is directly harmful. But others have acquired immunity against the invaders, and have even turned the invasion to account. Such are the pelagic tunicates and the cuttle-fishes. The best-known example of tunicate phosphorescence is that of the creatures known as *Pyrosoma*, which form transparent, gelatinous, tube-shaped colonies floating on the surface of the warmer seas. The walls of the tube are composed of numerous individuals seated in a common gelatinous envelope and adding to their number by budding. The mouth of each "person" is directed outwards, and close behind it is a patch of tissue which is the light-organ. It has been discovered that the cells composing this organ contain luminescent bacteria, and it is the glowing of these that gives the animals their phosphorescence. It is not easy to imagine what advantage the *Pyrosoma* colony derives from this; the animals have no eyes, they are hermaphrodite, and they lie in close association; but some important advantage there must be, for the eggs that will give rise to new colonies are always furnished with a certain quantity of the bacteria, handed on from the parent.

When the *Pyrosoma* individual is sexually mature, some of the bacteria in its light-organ begin to form spores, which then leave the shelter of the cells in which they have developed and are carried by the blood-stream to the little sac in which the single egg is developing. Invading the cells of this sac, they seem to induce these to divide, and one of the daughter cells at each division moves into the space between the sac and the egg. This invasion continues and the infected cells continue to multiply and move in towards the egg until there are about four hundred of them. The egg itself has meanwhile begun to divide, and the infected follicle cells, glowing all the time, take up their position between the blastomeres. Each egg gives rise to four *Pyrosoma* individuals, which will be the founders of a new colony, and between these four the invading luminescent cells are scrupulously divided, taking up their definitive position, as time goes on, in the light-organs. In this way, from generation to generation, the sacred flame is handed on.

In the cuttle-fishes, the eyes are in their way as perfect optical apparatus as those of a vertebrate, the sexes are separate, and in the majority of species the luminescence is shown by the female only. The eggs of the cuttle-fish are enveloped in a shell which is secreted around them on their passage to the exterior by structures known as the nidamental glands. In front of the nidamental glands lies another, usually called the accessory nidamental gland; and it was always supposed that this furnished some contribution to the egg-shell. But now we know that it does no such thing; it is a phosphorescent organ, composed of tubes of three kinds and colours, white, yellow, and orange, each of which is crammed with bacteria of a different sort; it is those in the yellow tubes that are luminescent. The luminescent gland opens to the sea and the bacteria can pass out. The cuttle-fish, then, may glow with a more or less steady internal light, or it may eject streams of fire. In some cuttle-fish the apparatus is further complicated by the development of a reflector behind the gland, backed by a pigment screen, and there is actually a lens in front, so that the animal has a veritable bull's-eye lantern. The opening of the light-organ is so arranged with relation to the genital duct that the eggs as they pass along get smeared by the expressed bacteria, and the new generation is safely infected. We find the bacteria glowing inside the egg-shell, though how the embryo actually incorporates them we do not yet know.

The presence of three different kinds of bacteria is paralleled by the condition of things in certain insects. It is known that many bacteria are mutually interdependent and will not flourish when isolated from their fellows; possibly we have here a second degree of symbiosis within the first. In these cases, then, the light would seem to be the product of captured and tamed bacteria; and we speak of a symbiosis, though we are very far from understanding yet the special advantages that accrue to the microbe partner.

Now, while the symbiosis productive of luminescence may give protective advantages or facilitate mating, the other and far commoner examples with which I propose to deal are concerned with nutrition. The primary concern of all living organisms is with food, the getting of it and the dealing with it when it has been secured; and we cannot even begin to understand the majority of symbiotic partnerships until we know something about the feeding habits of the organisms concerned. In the more intimate associations, as of green plant with fungus or bacteria, of animal and green plant, of animal and fungi or bacteria or protozoa, the microscopic partner has been called in to perform some function that the larger partner cannot perform for itself. Let us bear in mind that the green plant, the fungus, the bacterium and the animal have each very different capacities of dealing with the material that composes what we call their "food."

It must be admitted that, seen from our point of view, many of these associations appear very one-sided in their benefit and border closely on true parasitism, between which and symbiosis there is no hard-and-fast line to be drawn. Strictly speaking, we should use symbiosis to describe a condition where equilibrium is established between the partners, but we still use the term when one organism seems to derive more benefit than the other: true parasitism may be said to occur when the benefiting organism gets the upper hand so far that it lives actively upon its host's tissues or diverts so much of the available food that the host dies of starvation. Obviously, it is seldom to the advantage, even of a parasite, to kill the goose that lays such golden eggs; and where this occurs, we may assume that perfect equilibrium has not yet been achieved. In the course of ages many harmful parasites, as we see them to-day, may become innocuous; and as their hosts develop an immunity, they may even become useful symbiotes.

It is well known that the leaf-cutting ants of the genus *Atta* do not feed directly on the leaves they cut up, but use these as manure for their fungus-gardens, and it is on the white mycelial nodules of the fungus that they depend chiefly for food. The greatest care is taken of the fungus-gardens, and we may say that the same sort of symbiotic relationship exists between the ants and their fungi as between the ants and their green-fly "cattle." It has for a long time puzzled biologists to discover how the precious plant is transferred to the new nest when the young queen leaves the old colony; now it is known that she carries with her, in a little pocket under her chin, a sample of the necessary mycelium, and in the new nest she deposits this and cares for it as diligently as for the eggs she lays, until such time as the workers hatch out and are ready to take over these menial duties.

Strange to say, the same habit of fungus-culture is also found in one of the families of termites, and it occurs again among certain beetles, such as *Hylecoetus dermestoides*, the larvæ of which live in tunnels that they make in fresh wood. These larvæ, when they hatch out, feed upon the mycelial nodules, rich in protein, which line their tunnels. It has recently been shown by Buchner, that the adult female *Hylecoetus* has on her ventral side two elongated pockets filled with thick-walled fungal spores, and between these pockets lies a gutter also filled with spores. All these structures end just where the oviduct opens to the exterior, and the eggs as they are laid get smeared with the spores squeezed out on them by the muscles of the abdomen. The eggs are deposited on the bark of a tree, and the larva, in eating its way out of the egg-shell, devours with that the spores and so gets infected. The larva burrows into the wood, and the spores, passing through its body uninjured, are deposited in the excrement, germinate, and, even in the poor soil of the powdered wood in the tunnel, produce a flourishing supply of rich fungal food.

The wood-wasps of the genus *Sirex* do something of the same kind. Here the infecting apparatus consists of two syringes filled with the ooidia of a fungus, and between the syringes is a gland, the sticky secretion from which mingles with the fungal material as it is squeezed out when the eggs are laid. The mycelial growth that appears within the larval tunnels is never so rich as with *Hylecoetus*, and here it may be that the grub merely makes use of the fungus as an aid to the digestion of the gnawed wood, about 50 per cent of which is pure cellulose.

Cellulose does not occur in animal tissues, if we except the group of the tunicates, and there are very few animals that produce enzymes capable of splitting it up and putting it in a more assimilable condition. So far as we know, no vertebrate can digest cellulose unaided, and among the invertebrates the only established examples are those of certain snails, the shipworm, the crayfish, the earwig and a butterfly. Innumerable insects live on vegetable matter containing a high percentage of this indigestible material; although they seem able to make use of it, they secrete no cellulose-splitting enzyme that we can discover. The suggestion is that they call in the aid of fungi and bacteria that have this peculiar power. We assume, then, that the fungus-gardens of the ants and termites and of other insects with wood-eating larvæ, furnish not only direct nutriment but also substances that will split up cellulose for the animal.

From the external fungus-garden in the nest or the burrow, it is only a step to an internal symbiosis. Why not carry one's garden around with one all the time?

We find, in fact, that the majority of insects living on plant tissues or plant juices have outgrowths from the gut in which swarms of yeasts or of bacteria have their permanent abode. Sometimes, as in *Dacus oleæ*, the olive-fly, the symbiotes live free in the cavity of the reservoir. More often, perhaps, they are contained within the cells of which it is composed. How the micro-organisms are prevented from multiplying to excess we do not know; but it is what we should expect in a true symbiosis—that the host should have developed some power to keep its guests in useful check.

Here, as in *Hylecoetus* and *Sirex*, we find the most elaborate precautions for ensuring that the next generation shall be furnished with the necessary supply of the symbiote of the species. When the female insect is sexually mature, numbers of the bacteria or of the yeasts migrate to the hinder end of her body and take up their position in outgrowths from the gut opening just by the aperture from the oviducts. The yeasts are squeezed out on the shells of the passing eggs, and are presumably swallowed by the larvæ as they emerge; the still smaller bacteria frequently pass through the micropyle of the egg or through tiny pores alongside this, and the emerging larva is already safely infected.

It has also been observed that blood-sucking invertebrates habitually harbour micro-organisms, which may possibly help them to digest blood. Lice, bed-bugs, tsetse flies, culicine mosquitoes and leeches all have in their guts micro-organisms comparable with those we meet with in insects depending on a plant diet rich in cellulose. In some instances here also the transfer of the symbiote to the young of the host has been demonstrated.

It is not only in connection with luminescence or with their immediate digestive activities that animals have called in the aid of symbiotes. Certain snails of the families Cyclostomatiidæ and Annulariidæ have long been known to have curious branched, concretion-containing "glands" lying on the dorsal side of the intestine and in close proximity to the kidney. The concretions are spherical in form and are composed mainly of uric acid deposited in an organic matrix arranged in concentric lamellæ. They lie in special cells known as purinocytes, and alongside them within these cells there are almost always quantities of a bacillus. The purinocytes are undoubtedly excretory in function. The work of Meyer and others has shown that they remove from the snail's tissues and store the excess of nitrogenous waste in the form of the concretions. Then, according to Meyer, the bacilli invade the purinocytes and do their work, which seems to be the breaking up of the uric acid.

The tissues of the snail itself do not produce any uriolytic ferment, and the animal seems to depend on bacterial assistance at this point. A number of free-living bacteria are known to have this power of splitting up uric acid, and in the soil, among the decaying leaves on which the snail feeds, are found bacteria indistinguishable from those in its purinocytes. They also occur in the snail's gut, where they have come with the ingested food, and there seems every reason to suppose that they make their way thence to the excretory cells—though why they should show this special affinity for the purinocytes remains a mystery. (The same might be said of the yeasts in the "mycetomes" of insects.) Presently certain cells in the neighbourhood of the purinocytes become actively amœboid and devour the purinocytes with their contents—the organic basis of the partially dissolved concretions, that is, *and* the bacterial symbiotes whose work is now over. Presumably the phagocytic cells then hand over to the snail's tissues the broken-down products, and presumably these are anabolised by the mollusc, especially during the periods of inertness which we call hibernation.

I say "presumably." It will be noticed that in nearly all these recently investigated examples of suspected symbiosis, we must still qualify our assertion. The inference is strong that the micro-organism is a true symbiote. There is much circumstantial evidence—its constant presence in the special situations, its unvarying character, its scrupulous distribution to the offspring, its powers of producing chemical changes of which the host is known to be incapable, but can, in its presence, effect. But we cannot say with certainty that the partnership exists, in however one-sided a degree, until we have proved by experiment that the containing animal suffers irreparably through removal of the guest, and is benefited by its return.

An experiment of this kind has been undertaken and carried through with success in the case of certain wood-eating termites. Some termites habitually cultivate fungus-gardens, and such species live on rotting wood and other vegetable matter plus the assisting fungus. The true wood-eating termites, and these form the majority of families, cut up and eat wood that is quite fresh ; and termites of these families do not cultivate fungus-gardens. The wood on which they depend for subsistence contains at least 50 per cent. cellulose, and the experiments of Cleveland have proved that such termites, kept in the laboratory, can live for at least three years—perhaps indefinitely—on a diet of pure cellulose. In these experiments of Cleveland's, the cellulose was given in the form either of pure filter-paper or of specially prepared ligno-cellulose.

The cellulose-fed termites in the laboratory behaved in exactly the same way and flourished just as well as the controls living on a more normal-seeming wood diet. That is to say, the workers always fed directly on the pabulum, and so did the nymphs of all the other castes. The royal forms likewise fed themselves until the so-called post-adult stages, when they, together with the second and third form adults and the adult soldiers, became dependent on the workers for food-supply, the muscles of their own jaws atrophying, or, in the case of the soldiers, the mandibles becoming so large and unwieldy as to be useless for wood-gnawing. The dependent castes fed either on the semi-digested food passed from the hind-gut of the workers, or on the secretions poured out from their salivary glands. It was the soldiers who seemed to live most constantly on the semi-digested gut-contents of the workers; the younger creatures—the nymphs and the royal and complementary forms in their later life—depended on the salivary secretions.

Now cellulose is indigestible even by termites, which secrete no cellulose-splitting enzymes; and these families have not even got fungus to aid them. But it has been known for a long time that the gut of the true wood-eating termite that does not cultivate fungus, harbours an extraordinary menagerie of protozoa not found anywhere else, if we except some small relatives from the hind-gut of the cockroach. Unless it has been seen, the writhing multitude of inter-sliding protozoan bodies that almost blocks the gut of a healthy termite worker and constitutes about half its total body-weight, is difficult to picture. In spite of their relatively large size and the vast numbers of motile threads covering their bodies, they are ranked by protozoologists among the flagellates, where they form a special and peculiar group, the Hypermastigina or trichonymphids. It seems that each genus of wood-eating termites has its own special association of trichonymphids. Now it is to be noted that the flagellates are found abundantly in all the castes at the stages when they do their own feeding. They disappear from the second and third forms in later life, and become less abundant in the soldiers after these have passed the nymph stage. Larvæ isolated from the time of hatching never have any; but if they are placed with workers, they have protozoa in their guts within twenty-four hours. The soft protoplasmic bodies of the flagellates are generally crammed with tiny fragments of termite-masticated wood which they have picked up: they have no mouths, but probably take in the particles at the naked posterior end of the body.

It has long been suspected by protozoologists and by entomologists that these strange flagellates are not parasites of the termite, nor even mere commensals, but true symbiotes in the highest degree, conferring incalculable benefits on their hosts, and, richly compensated in return by food and shelter, become incapable of living a separate existence. It has been suspected that these protozoa, like certain fungi and bacteria, have the power of splitting up cellulose, living on the more assimilable products and handing over to their hosts a certain proportion thereof, adequate not only for the particular individuals they inhabit, but also for the dependent castes.

Cleveland's ingenious experiments have recently carried these suppositions into the realm of scientific fact. First he set about removing the protozoa from their termites without injury to the insects. This was difficult. He did it in three different ways—by starvation, by keeping the colonies at a temperature of 36° C., and thirdly, by subjecting them to oxygen under pressure. The first method, starvation, removes nearly all the protozoa in about fifteen days, but it is impossible to defaunate the insects completely before they themselves have begun to suffer in health. Incubation at 36° C. for twenty-four hours kills the protozoa without damaging more than a small percentage of their hosts. But an oxygen pressure of four atmospheres kills the flagellates in about half an hour without damaging their hosts at all, and this method has been found the most convenient for experiment. The various kinds of trichonymphids in one termite gut are variously susceptible to the effects of the poison. By varying the dose and the period over which it acts, Cleveland has found it possible to remove first one species and then another, thus altering the character of the particular intestinal fauna in which direction he will, for when one species dies out another there present multiplies rapidly and takes its place.

The termites defaunated by the oxygen poison are themselves perfectly healthy, but when they are supplied with wood to feed on, though they devour it greedily, they cannot digest it, and they die of starvation in three to four weeks. Supply them, however, with predigested humus or with fungus-digested cellulose, and they can get on all right. But the crucial test is yet to come. Put them back with other termites of their own kind containing protozoa; they rapidly become re-infected, are then once more able to cope even with pure cellulose, and can live on that indefinitely. There seems, then, no question whatever that the protozoa split up the cellulose for them, and that, in the course of ages, they have become absolutely dependent on these secret sharers for their essential food. The flagellates, for their part, cannot live for more than ten days when removed from their termites, and then only in a special blood-serum medium to which finely-powdered ligno-cellulose is added. They have never been known to form protective cysts, and, so far as is known, they do not occur anywhere else in Nature. The exact method by which they are transferred from termite to termite is not fully understood—though probably they pass in the semi-fluid substance from the ani of the workers.

The association between these partners is undoubtedly of very long standing—it must have taken many ages to evolve the exact adjustment between them and the extraordinary specialisation that we find. But complete and successful the partnership undoubtedly is. Many minor details have yet to be worked out. We do not yet know in what form the broken-up cellulose is handed on to the insect. A great deal of glycogen (animal starch) is always found in the bodies of the trichonymphids, though none occurs in the intestinal cells of the termite. Even when the diet has been pure cellulose for as much as three months, the protozoa still contain glycogen. The suggestion is that they split the cellulose into the sugar glucose, which they then build up into glycogen. How they hand over the excess to their partners we do not know, or whether, as seems possible, their own bodies are sacrificed in the process. Nor do we know yet how the termite gets the nitrogen necessary for the formation of protein when it is fed on pure cellulose. Possibly the bodies of the junior partners afford the immediate supply: but whence have *they* got their nitrogen? Have they the power of fixing free nitrogen, as certain bacteria have? Or do the termites themselves perform this un-animal-like feat? Do not let us forget, however, that along with the flagellates in the termite's gut there are also myriads of other micro-organisms—spirochætes, bacilli, and what not. It may be that these are, in their degree, essential partners in the process.

[D. L. M.]

WEEKLY EVENING MEETING,

Friday, May 18, 1928.

SIR ARTHUR KEITH, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

MORLEY ROBERTS, Author of "Followers of the Sea," etc.

The Sea in Fiction.

To tackle what might seem so light a subject as sea stories in a house which for me is the very home of English science would appear to some almost a desecration. If by a trick of my own mind I could not relate such a discussion to those of a much weightier character I might have humbly declined the great honour of addressing you. But after all whole volumes could be written about it, and we are so essentially a sea-faring nation, having, as high authorities assure us, sprung from races which played and fought for tens of thousands of years on the shores of the Baltic, that anything to do with the sea has a very vital connection with the British mind and the nature of our mixed maritime race. I trust no one will think so solemn a beginning preludes any arid discourse. I say it merely for my own satisfaction and to mollify the manes of this great Institution. For big as the sea is, and much as we owe it, I have to own that when I put out of my mind Hakluyt and history and turn to tales of the sea I am much more apt to ignore its tragedies and to take it humorously than may seem respectful to some. If I may suggest on such an occasion as this, when I have to deal with great and little writers and their works, that I have written sea tales myself, you may also allow me to quote the saying of a second mate of my own drawing who, on being told that another seaman then ashore did not get on well with his wife, remarked with indignant surprise, "Why doesn't he go to sea? What's the sea for?" Now the notion that the sea was primarily intended as an avenue of escape from domestic trouble may be a little limited, but so, for the greater part, is the theatre of sea action when we have to deal with it in novels and stories, though I shall have to speak of one book which might seem to the imaginative an allegory of man and the very universe itself.

Now, as the actual ship is a narrow scene and the sea often monotonous, their value in imaginative narrative of course depends entirely on treatment. And I am sorry to say that the bulk of what I have read is not very good from a seaman's point of view. All fiction has its conventions, but they must be consonant with some degree of truth, so that, granting them, the rest follows naturally, all "ship-shape and Bristol fashion," as the old sea-dogs used to say. But the conventions of bad fiction tend all the time to get away from what we call "truth." And "truth" here and in all fiction may be defined as that which does not outrage the feelings and contradict the knowledge and experience of those acquainted with the subject. I should be much more inclined to dilate here on bad sea fiction if it were not that special knowledge is needed to rage at or be delighted with the amazing errors and howlers of those who write without knowing their subject. Yet, even so, many of you may smile at the romantic story-teller who wrote a thrilling sea-yarn in which a young second mate occupied himself during the height of a cyclone in skipping about the decks taking snapshots. And only lately I read an apparently naturalistic account of a calm at sea during which the captain and officers were much exercised to keep the crew "amused" (yes, that was the word!) as the poor fellows had nothing to do! Accordingly these officers organised games and fishing parties, and, as one may guess, sent a wireless message to all sorts of fish to come and be caught. Those who have laboured and sweated in a calm in the doldrums or about the line will, according to their nature, laugh or be angry. Let me tell you, however, that in respect to his "mystery" and its language and conditions the real old sea-dog, the veritable shell-back, is as terrible a conservative as any die-hard, and cannot be amused by any such ignorant idiocies as those I have mentioned. This strong characteristic of those who follow the sea is especially seen when anything is written which conflicts with the traditional relations of the men or officers with their superiors, and it is roused even more strongly when the sea is regarded as something very romantic. This I shall deal with later when I come to speak of Conrad.

It may be taken for granted that nearly all modern work, especially that seen in magazines, is essentially bad in the sense that it is false. However well written and however popular it may be, a seaman will turn it down. I do not propose to criticise living writers, and shall therefore conceal the name of one whose "popular sea-book," as the critics called it, was condemned as utterly silly by a well-known merchant captain and his officers, and, after having had that judgment written on its fly-leaf in the strongest maritime diction, was put into a tin box and solemnly consigned to the sea in Turk's Island Passage. Just lately I picked up a novel, largely about sea-life, by quite a clever writer, and having opened it at a passage which showed a reasonable amount of knowledge I turned to another in which all my feelings were so outraged that I became speechless. The indefatigable novelist had evidently read up his subject! Is there anything more fatal? The result at its best is mechanical and lifeless. No one but the seaman can say how easy it is to fall into errors. Even he himself when far from those dear old ships which now are so rarely seen, and to-morrow will not be seen at all, can make real technical mistakes. Let me confess with great shame that in a story I once stowed the mizzen topmast-staysail in the main-top when I should have stowed it in its proper net. I gasped when I read it in print. Of course, as you doubtless recognise, I meant to write the mizzen t'gallant staysail! But such mistakes are easy when writing hurriedly, though they are not fatal if the atmosphere is right. The uneducated unsalted sea-writer always gets that wrong. Do we not observe the same when journalists touch upon science in any form? Some of you may have seen not long ago the report that Mr. So-and-So died of "a very fatal complaint, meningitis of the stomach." It is true that there is an old sea yarn of a Greek who said "different ship, different fash," when he went aft to stow the jib, but a ship is still a ship, and the seaman so loves her that any ignorance displayed by those who pretend to know her excites his loftiest indignation. Let any ladies here read, as I have read, of women turning up at a garden party in evening frocks and how much farther will the book carry them?

Yet there are those who say that such ignorance really does not matter. Perhaps it does not when a city man, say, picks up a false sea-story. But let him take one that deals with what he knows and gets it all wrong, what will he say then? I have observed lawyers almost blue in the face when reading stories that touched on legal matters. So it is with doctors. So it is with all of us who have any technical knowledge. These errors *do* matter. They are in a way fraudulent. We have bought a story and have been cheated. Why should sailormen have their deepest feelings thus outraged? Is it not that garden party over again?

Now as I have had some little personal acquaintance with ships, especially with the foc'sle of ships and the many ways of the sea and seamen of all classes, English, British if you prefer it, Dutchmen, Dagoes and even Lascars, I am often asked to "look over" and "put right" books written by those who want to delineate some part of a sailor's life. In every case the mistakes made might with much labour be corrected, were it not that in all the whole psychological background is somehow for ever fundamentally false. Put them right indeed! How can one render righteous that which is shaped in the iniquity of ignorance? Once a well-known novelist, not assuredly of the first or second rank, asked me to "look over and put right" the three first chapters of a new book. I reported that they were utterly wrong and must be altered. "Why, I should have to rewrite them?" he said indignantly. "Exactly so," I replied. "Well, I shan't. The public will never know." And they were printed as they stood. In other cases I have done my best and failed utterly. But the writers could not believe that a seaman would turn their books down about five minutes after they took him to sea.

Is then technique so important? I think the answer is "yes" and "no." A genius can write what he likes. Oliver Madox Brown's *Black Swan* is in its way great. I do not care if the ship "rocked" instead of rolling. Nor do I suffer when I find that folks can run about in the 'tween decks and the hold when she goes to sea. But we are not all geniuses, and the less we are of that order the more careful we must be. Can I not imagine some of my old sea-mates reading a book of Jack London's in which he calls the heel of a mast the "butt"? Good heavens, what an outcry. Are then ignorant men such as these judges of literature? No, but they are competent judges of the competence of the writers to write about the sea.

Now, if I have so far said enough of these, what of the good men and true with salt in their bones and on their sun-tanned cheeks, even on their whiskers in the days when whiskers were worn, and the stories truly told of them? It is a very old subject, the sea. Some of you may remember in the history of romance, of romantic writing, the part that the Mediterranean played. Pirates came and took away the fair damsel. Even these pirates of romance seemed to have behaved romantically, for she usually survived to be rescued by her lover. One may say rightly that sea fiction began with the Odyssey. Are not all the adventures proper to romance in that gorgeous sea-story? Then, long long afterwards, followed the post-classical romances, such as *Ephesiaca, or the Loves of Anthea and Abrocomas.* I will not bore you with its details, but here you have pirates and the sea and love, sometimes, as nowadays, unduly prominent. But it is interesting to note that we may say the whole of modern fiction flowed from the Odyssey, that high spring upon Olympus. Yes, even Balzac and those who write psycho-analytical novels, as well as those who pen good and true sea-tales, may touch their forelocks to our mythical Homer and his sea-captain Odysseus.

With such an ancestry may we not get mingled results? There are some great names in the written romance of the sea. Presently I shall tell you what I think the greatest sea-story. In it there are the surge and thunder of the sea and the surge and thunder of a very strange mind. But we cannot pursue all sea romance through the paths of ocean, and therefore with this book in my mind I come back again to works in the English tongue. The Greeks are still fine seamen, though liable to panic in great emergencies, but they have not adorned sea literature since the days of the romances of which I have spoken. In France there has been one sea-writer, Loti. As a writer about the sea or the sea mind I cannot rate Loti very highly. There is a morbidity and unwholesomeness about him which I often find unpleasant. He writes about himself rather than the sea. I am told he was a good and keen seaman, and find it hard to believe. The salt-laden breezes of a sea life ought to have blown out of him his unpleasant subjectivity. Victor Hugo at sea is out of his element and would be if one did not recognise that he is merely acting somewhat flamboyantly in a theatre with a painted backcloth of ocean. It is in the English tongue that the greatest sea-stories have been written. And how curious it is that one of the best-known of all should be by an anglicised Pole. You know that I mean Joseph Conrad.

Just lately those of you who read the newspapers may have noticed that Mr. George Moore has been so kind as to tell us, by a pleasant implication, that he is the only real writer left to the English world. This he did, beautifully in indirectness, by saying that the late Thomas Hardy was not a writer of any importance whatever and that Conrad was nowhere. Hardy, who knew not the sea but found an almost equal grandeur in a silent and sombre moorland, can even when dead take care of himself, and in *The Dynasts*, *The Woodlanders*, and some of his poems, of which I would remind you only of *The Pyramid of Cestius*, he has a great and enduring defence. This is beside the main question, I own, but to speak of Conrad is here part of my task, and if I say in what he failed, or in what personally I think he failed, you must not for an instant believe that I regard him in his best work and highest moments as anything but a great writer.

Now it is the business of a seaman and the result of his training to be ready for all emergencies, wind or fog or fire or wreck, and with that alertness of mind which is his very foundation he must be skilful in ready adaptation so that he can use any tool that comes to his hand, whether it was meant for the task or not. But in languages we cannot say the seaman is so wonderful. He can, of course, as a rule, swear very effectually in many, though somewhat crudely, and not with the magnificence that seems proper to the sea. Conrad was not English and yet he took English as a tool and mastered it, or so nearly mastered it that the flaws in the web he wove are strangely few. If some of his best work is not remembered when Mr. Moore's alembicated and sophisticated efforts to be remarkable through the ages are not wholly forgotten, then am I greatly mistaken. When at some future time Mr. Moore's purified spirit ascends again to the cool airs of this earth it may be chastened even more by that than by any possible purgatory.

What is it we seek in stories, in stories of the sea? I would not here and now lie to and philosophize with the open sea before us. But do we not seek always to know the lives and ways of men and living things or those elusive elements to which we deny a life? Here, in this house, you have heard strange stories of atoms and electrons, of the ways of living cells in the body, and what you heard was something of the way these live, what they do and under what spell or ancient order, or inward inexplicable energy and impulse, they act. So all of us who have that vital intellectual curiosity which preserves us from premature hebetude and torpor desire to know our fellows. Such curious creatures are we in our lives, and their secret reticences, that we perceive all others too, of whatever order, have their own, and feel that over and above the daily outward life of each is something we may understand to our advantage and satisfaction.

What then, again, do we seek in stories of the sea? I say nothing of those who read merely as an anodyne. I think we want to find out what the lives of seamen are. We have an inborn curiosity about our native sea. We want to know the secrets and springs and purposes of the heart of the seaman. So what is his heart? Conrad tells us much, and sometimes, as I shall say, too much for him to be English. And as we try to learn the truth we find seamen very silent, for the Englishman and the Scotchman are always just a little loathe to let anyone see what they feel about themselves, and even if they do love the sea and feel its magic, cannot bear to own that they do. In many things Conrad can help us in that, just because he was an alien.

For here I must explain why it is that by and large, as seamen say, he does not appeal to them so much as he does to landsmen. Often I read him with a curious uneasiness which I have found in others, and this in spite of the truth with which he depicts man and the elements. There are stories by which I am not disturbed. Take for a moment his *Typhoon*. A gale at sea, a real gale, not one of those about which lady passengers love to tell us, not one of those which a complaisant captain with his tongue in his cheek says is the worst he has ever known, but a real gale, a "whole" gale, and more. For a typhoon, a cyclone, or a hurricane is one of the greatest and most terrible of natural phenomena. Many of you have felt the wind and heard it cry aloud and have felt it shake the house, but not many here have known the best and worst it can do. Conrad knew and depicted it in that tale of the stubborn old sea captain, who, ignorant of the nature of a cyclone, held on to its centre and then again emerged into the hell that circles around the vortex of that great whirlwind. I have myself been under some such domination in an old sailing ship, and have known what it was to feel that the end might come at any moment. The blind fury of the elements that deafen men and blows their very words out of their mouths is drawn wonderfully by Conrad. Read the tale again and you will know much. None can know all until they have themselves felt the wind as something alive and dreadful that would, so it seemed, tear one from the deck and send a living body to leeward like a wisp of dry spun yarn. Even on the land in a tornado I have lain for three hours holding on to the root of a bent and broken little tree, and have wondered how long I should be left to lie there. If I could write of that as Conrad wrote of a cyclone I might hope for some remembrance.

Yet if Conrad can do this, why have I said that I often read him with uneasiness? It is difficult to explain to any but a seaman. To some of them, who scarcely understood why they did not wholly like him, my theory has commended itself. The truth is that Conrad, good seaman as he was, could never by his nature be an Englishman. He was to the last day of his life a Central European. For mark this, no man can get away from his ancestors, and Conrad's were those who had no recent salt in their bones. How does the Englishman look upon the sea? He knows instinctively and without knowing it that the race of man sprung out of it. He has had no long centuries apart from it. He cries for it in his youth. He even goes down to Brighton at the end of the week as if he were going home. My old friend W. H. Hudson, whenever he got to the sea, scooped up and drank a handful of salt water. I know an artist who when slightly exhilarated by alcohol, walked into it, clothes and all as soon as he came to it. "Why do you do that?" asked one of his amazed friends. "You are a fool and don't understand," said the painter as he sat down in the water. The sea to a seaman is the place where he gets his living. The ship in the old days was his house. Sometimes he adorned his poor narrow bunk with photographs and pictures that he might be all the more at home when at sea. "Romance of the sea," said an old mate to me once, "is all bosh. I get my living by it, and it's as romantic as driving a bus up and down Whitechapel Road." That is an extremely arid view, no doubt, but it shows something of the seaman's mind. That old mate could admire things as much as anyone, but he would rather have been drowned than admit it. His description of a cyclone would be what he must put in the log book. He would compress all hell into three lines and end, perhaps, with "John Smith and Thomas Robinson washed overboard."

Does not this explain such a man's attitude to Conrad? Conrad is always surprised by the sea; it is always wonderful and "austere." A fine word "austere," but it takes austerity to find it in the ocean. Our seamen are most decidedly not austere, and take the sea as it comes; it is their job to beat it or be drowned, and up to that point to make a jest of it. Conrad gives himself away too much. He is always self-conscious. He is never at home at sea. For him the sea *is* something "to make a song about," to "write home about." The average seaman never makes a song about a "breeze," and at sea a breeze means that moderate gale which is described by passengers as "a dreadful storm." This attitude is not mere stupidity or insensitiveness; it is dictated by humour and knowledge, knowledge inborn as well as acquired, and however much Conrad learnt and observed he had neither humour nor any inborn salt in him. All the same he remains a great writer of the sea. Absence of humour is not fatal to a writer. If it were—what of Milton? And as I am, as it were, damning false romance and over-sensitiveness, let me go back a little in the centuries to very grim realities.

Tobias Smollett must have a word or two, for though he never wrote anything but scraps and chapters about the sea, he has the naval atmosphere of his time in all its brutality. A Scotchman and a surgeon of his savage and contentious character was hardly likely to view things naval in a roseate light, but all the same we have to recognise in every word the character of truth; in every seeming caricature a portrait. What ships and men were in Smollett's time may be inferred from far later documents. The log of Nelson's *Victory* just before Trafalgar contains repeated instances of men being flogged in batches for drunkenness. Smollett was a man of letters by nature, that is, he had the seeing eye and the urge to put down what he saw. A realist or naturalist, we may call him. He said what he saw "without mitigation or remorse of voice." But coarse as it often was he had humour as we commonly think of it, and also that which is full brother to pathos, as one can see in his picture, a very living and dying picture it is, of Commodore Trunnion "taking his last departure." Those days have gone; the old pig-tailed seaman is no more, and yet we may sometimes look back into our own sea history with profit. The state of affairs I knew fifty years ago in undermanned, underfed merchant ships must have been paradise compared with life aboard King's ships in Smollett's time. Nothing but the inextinguishable humour of our men could have made life possible. So there is no romance in Smollett. Yet if it is grim enough and foul enough and coarse enough we may be thankful that over all is the glimmer of humour, and at times, rare times, the dew of pity. Romance, ah, how shall we get it in such conditions except by falsehood, by suppression of the truth and the suggestion of the lie?

You may wonder to hear this said, for is not the stock-in-trade of the unsalted sea-writer the words "romance of the sea?" Shall I tell you how it comes that in the seventies even there could be little romance for seamen, save for some youth fresh to salt-water and full of the inextinguishable love of experience and adventure? Let me depart for a moment from fiction, and give you a little picture of the sea-life as late as 1879. Do not forget that such a picture might be part of a novel. We lived for'ard in the foc'sle, a dim and dark den often with no light, for when the weather was bad the scuttle just above our heads was closed. We had bunks ranged one over another on port and starboard sides; narrow enough coffins with a "donkey's breakfast," otherwise a bed of rough ticking and straw, for us to sleep on. To sleep when we could! In the hot weather, bugs; in the cold weather, cold; in the rough and wet weather ceaseless and unconquerable damp. There was no door to the foc'sle I am thinking of; nothing but a canvas screen that waved and flapped in the wind. When she dived and shipped a sea a cascade came in over the bows and poured down on the deck. As she dived again the sea inboard rushed for'ard as we skipped upon chests to avoid it. Maybe we did not avoid it. The two hawse pipes, up in the "eyes of her" for the cables, were plugged ineffectually. As she hit a sea water squirted in on us with the force of a hydraulic jet. So once and more than once my mates and I have been wet through for a fortnight. There was no drying of clothes. My own mate and I wrung out ours as we stripped when going "below," and took the warm wet things to bed with us into wet cold blankets so that they should at least be warm when we turned out. When we did each bunk steamed, for we had been sleeping in a cold wet pack that we had made a hot one. Maybe in very heavy weather, or when it was coming on, we got little chance of sleep. No sooner were we hard at it than we heard the shrill pipe of the bo'sun and the inevitable call of "all hands." In the old days before that time they seem at least to have had enough "beef to the braces" to handle a ship even with one of the watches. But in the seventies it was all hands whenever a topsail had to be reefed or picked up. So I can remember spending ten hours aloft out of three four-hour watches, two of which it should have been my turn to stay below. And the food? Well, you have heard of salt horse and the old verse which begins "Salt horse, salt horse, what do you here?" Hardest beef, beef or horse, quien sabe? and hard tack full of maggots, and reasty pork and, the gracious gift of the then Board of Trade, those awful things known as preserved potatoes, and tea, or tea dock sweepings, in a bucket and our own tin plates and pannikins, and, once in a blue moon, if the skipper and the steward were alike gracious, the salt scraping of an emptied butter-cask. Of course it was not for ever like all that, for long months of it, as we used to say, would have killed a brass monkey. Forgive me these phrases. They come out of an old sea-chest of memories. We had our decent times, if the officers were decent men. In fine weather, in the trades, when we touched neither sheet nor tack for weeks together and there was shade here and there to work in, life was pleasant, if it was an English ship. In American ships there was rarely any good time. I could wring your hearts, I know, with stories, true and awful stories, of what happened on board many American ships even in my time. The one thing they did well was to feed the men. But, by all the gods of the sea, they took out of them more than they put in!

It is true that nowadays, save in disaster, things at sea are rarely so peculiarly lacking in comfort. For now men will not stand so much, and wise shipowners will not try them, while the noblest and best of shipowners would not if they could. Whatever is done for the seaman, when true tales are told of him so that his life is known, nothing can do away with tragedy at sea, tragedy sudden and terrible, or most awfully prolonged. The passing of the noble square-rigged ship in her majesty cannot do away with disaster, and if the old comedies seem doomed to sink with her, other tales must remain to be told, if they are not so naturally linked with the immemorial past, out of which the sailing-ship emerged by a true path of evolution.

Yet what about romance with a tale such as I have told you? Do we not want both humour and forgetfulness even to spell the word? True that even I have in one book at least taken a partially romantic view of sea life. But in that I used youth—a youth long since departed, or shall I even say, dead? Perhaps, like all of us, I over-rate my own humour. Had I had as much as the best known and most popular and most loved writer of the sea I might have reconciled myself more easily to tales I cannot read. Of course, as you guess, I speak of Marryat, the very king of the humour of the old navy. The great classic of sea-humour is *Midshipman Easy.* I care not who denies it, if any can. It is true that here and there I come across some one, usually a serious-minded woman, who cannot get on with it. And yet, though there is some knockabout fun, it is never coarse. There is none of Smollett's rude and brutal realism, but a realism such as springs from a joyous but also really observant and thoughtful temperament. Besides mere jests, we find in Marryat much wisdom, knowledge of men as well as knowledge of the weather and the ways of the sea, and he can also make us sorrowful. Are there now boys who have not read Marryat? I wonder if there can be. How many did he send to the sea? Many of those found that in fiction we are apt to compress adventure and the episodes which lighten the seaman's daily toil. But all the same, the sea took hold of them and made most of them its own. There are books which I read, or at least look at lovingly once or twice a year. Among these, not the least loved is *Midshipman Easy.*

Have I yet said which is the great sea-story? I do not speak of the true narratives of our marvellous old Hakluyt's collection, though indeed that is the great sea book of all time and all the world. But is there indeed a great sea-novel or story, anything which surpasses a mere tale, or even the true and sombre picture of Smollett, or the gaily-coloured truth of Marryat? Yes, I think there is. It is now many years ago, well over forty years, since W. H. Hudson and I discovered *Moby Dick* for ourselves, I know not how. I have lived to see it made a classic of a tentative kind, as it can be found now in the great *Everyman* series, and purchased for a song. How many of you have read it? I wonder. What you will find there is something, to my mind, very marvellous indeed. Sometimes we hear of Americans asking when the great American book is to come before the world. They own Hawthorne and his *Scarlet Letter*, Poe and his strange tales which the world knows of, and Walt Whitman, that even now only a few really value at his worth. But where is the great book? They do not know that it was written long ago when Herman Melville gave them the story of the great white whale.

Now this is a book of a kind for which we have no name. I am told that some folks reject with scorn great wines that come from such far-off places as Australia because they cannot truly be classed as wines of the Medoc or of Burgundy. This book is a novel, and more. It is a romance, and more, for it stands alone. It is a book of the whale, and more, for it is a book of man: a book of the sea and a book of the world. In English literature we have at least one book which, by its peculiar and almost monstrous and overwhelming character, has no class, no true peer. I mean *Wuthering Heights*. In that strange work of a sick and wonderful woman, the wind sobs in the hills and crags and is mingled with the cries and passions of men and women. A strange chorus, and one that sounds again in our ears whenever we remember Emily Brontë. There is no such picture of the common, though magnificently heightened, passions of man in *Moby Dick*, and yet in it we find an equal and more restrained power. As I think of the characters, true, though strange, intelligible, though gigantic, I wonder whether it is, or should be thought, a book of the sea at all. For the oceans are its background, and the ship is, as it were, a world apart, and it may be that the demoniac and dreadful white spermaceti whale is the fate we pursue and would overcome, the fate that in the end consumes and destroys us. Whatever kind of book we think it, or whether we think of it as a great thing standing for ever without likeness or a peer, it is at least the work of a seaman, a man before the mast, a man who went whaling, who knew the sea and its work and his own work, and was, in this book at least, a great genius. You may know the story of the mad, or at least maddened, Nantucket sea-captain and whaler who chased about the round world the great white spermaceti whale, which at an earlier period had torn off his leg. A queer, strange thesis on which to build a very pyramid of a book. Captain Ahab is perhaps almost monstrous, and yet we believe in him. We sympathise with him and with the harried and tormented and enraged whale himself. Himself I say, not itself, for this whale is more than a mere whale. He is a power, a force, and intelligence, and in the end looses the knot of the tragedy by destruction and victory. But who can tell the story? The characters tell it: the writer himself, a dim foc'sle figure, the Captain, Starbuck and Stubbs, the mates, Queequeg, the Indian harpooneer. Note that among whalers he is no harpooner merely. And there is the little boy, the cabin-boy, who soliloquizes so strangely and yet so credibly that we wonder. But is not the essence of great writing the power of making us believe, we know not why? We get faith and there's an end on't.

Whaling was once the great sport; now it is sheer murder, for it is done with bombs, not mere harpoons, while pumps inflate the dead whale lest he sinks after death. In Melville's time it was a fearful sport—cruel, dreadful and magnificent. That Melville knew. He gives us pictures of the whale in all his ways. He peers into the depths, and sees the mother whale with her infant just born; or sees it suckle, and he is often sorry. His picture of the gigantic, partly maimed and often wounded Moby Dick, with his savagery and swift intelligence and awful wrath, is something without its equal in sea literature. Read, I say again, and consider whether with all its faults the book does not stand out as a real masterpiece. Faults it has. Melville gave way too much to his lyrical spirit, and often confounded poetry with pure prose, so that he wrote neither, but terrible hybrid stuff some ill-educated folks think poetic prose. And yet he could write prose of a high rhetorical order, or of the greatest simplicity, and he saw deep into the hearts of men. I shall quote but one short passage to show how he could command and order words, so that they fell, as fragments of shining glass fall in a kaleidoscope, into wonderful patterns. This passage follows one which describes the certain terrors, of which Melville was a master, which came upon Starbuck, the mate. In the beginning of this passage he depicts man as he sometimes seems to be, and indeed sometimes is, as grand and clad in an august dignity. And then he cries out, perhaps in what you will think over-weighted and extravagant rhetoric :—

"If, then, to meanest mariners and renegades and castaways, I shall hereafter ascribe high qualities, though dark: weave around them tragic graces: if even the most mournful, perchance the most abased, among them all, shall at times lift himself to the exalted mounts: if I shall endow that workman's arm with some ethereal light: if I shall spread a rainbow over his disastrous set of sun: then against all mortal critics bear me out in it, then just Spirit of Equality, which hast spread one royal mantle of humanity over all my kind! Bear me out in it, thou great democratic God! who didst not refuse to the swart convict, Bunyan, the pale poetic pearl: Thou, who didst clothe with doubly hammered leaves of finest gold, the stumped and paupered arm of old Cervantes; Thou who didst pick up Andrew Jackson from the pebbles; who didst hurl him upon a warhorse; who didst thunder him higher than a throne! Thou who in all thy mighty earthly marchings, ever cullest thy selected champions from the kindly commons: bear me out in it, O God!"

Now it is the work and delight of writers to use and order words. However curious in over-wrought rhetoric this passage may seem to you, none can deny it peculiar power and the strangeness of some new beauty. It has often been said that there is for ever something strange and disturbing in all beauty that we see for the first time. But consider Melville writing it, and rising from the end of the chapter in exalted satisfaction. Here was the one time cabin-boy, the poor foremast hand in a whaler, the captive of wild South Sea Islanders, become the king and commander of great words. Yet as in all literature there is descent, a pedigree—as nothing springs full-armed from nothingness—it is a matter of real literary interest to discover that this passage owes its origin to the last writer in the world you might think Melville cared to read. For it was, it must have been, suggested by Laurence Sterne, of all men. That cannot detract from Melville any more than the fact that Keats owed, for its inception and very conception, his *Ode to Pan* to the influence of a little lyric in Fletcher's *Faithful Shepherdess*.

Some think there is another notable book in English about the pursuit of the spermaceti whale. I am loath to speak of it, but something I must say. Any book about the whale which even approached feebly to the order and character of Melville's book was quarried from it. For *Moby Dick* was till lately little known, and it was most unduly used. Of this I shall say no more, but I cannot bear to think that any, alive or dead, should wrap himself even in the torn hem of Melville's robes.

After this, which I say reluctantly, we may think of some other writers, who were at least honest men and did their work as well as might be. I have no sea-praise or sea-salt to award Clark Russell. His work is not really "true." His sea psychology is almost always wrong. Whenever you find in any sea story one of the officers familiar with any of the crew, or openly sympathetic with any of their grievances, you may put the book down as not of the real, but of the invented, sea. And when description is repeated and made a feature of, distrust it. It was written for the public and not for its own sake, as all true literature is or ought to be.

I have little to say, after mentioning Smollett, about the Navy. Though I did write one story about an Admiral, I know nothing of the Royal Navy. Merchant Jack is always more or less at enmity with the "matlow," for where he is overworked the blue-jacket has more to do a job than can get at it, unless there is fighting on hand. But I may speak of one book which deals, and deals very well, with the sea generally and with naval officers of a time now passed by. I refer to Cupples' *Green Hand*. There is real knowledge there, though I regret to say that he depreciates the merchant seaman most unjustly in order to exalt naval officers. No doubt there were very rough dogs among merchant seamen, but I am told that even now there are a few rough dogs left in the Navy, and the elder service, the one which grew up with the country, contained, as it still contains, thousands of splendid and capable men. Still the *Green Hand* is well worth reading, for though it too is overloaded with descriptions of maritime scenery, they are not fakes but observed and real pictures. Their one fault is a literary one. They are out of place in the mouth of a narrator, a thing too often forgotten by writers.

Something I must, of course, say about R. L. S. Stevenson took uncommon care about what he wrote, and obviously had the sense to get folks who knew to look over what he had done, so he rarely makes any of the absurd howlers of which I have spoken. And yet, so many are the sea traps open to the wisest, he fell into more than one of them. When I met him in Apia the very year he died

I asked him how it was that he came to write in *Treasure Island*: "'Luff," said he, 'and I put the helm up.'" His reply was that of Dr. Johnson when he was asked why he defined a pastern as the knee of a horse, "Ignorance, sir, pure ignorance." According to an old foc'sle gag he was in the same quandary as Moses, for that runs: "The Lord said unto Moses, 'Luff!' but Moses, not being a sailorman, put his helm hard up, ran his stem on the bulrushes, and became a total wreck." Again, Stevenson plays hanky-panky with the compass, and even in *The Wreckers*, when he had more knowledge, or better help, he makes very obvious errors. Yet in that book the maritime parts are on the whole very good, and give us some true pictures of the sea and of some American seamen of the seventies and eighties. Brutes they were, almost without exception, but they knew their work and the medium in which they lived and worked.

And now again what of modern and current sea-fiction when it is not the pure invented nonsense one finds in magazines, of which I have spoken with such tender moderation? Well, you may take it for granted that so far as the literature of sailing ships is concerned almost anything written is, in the slang of the studios, pure fake. However good it may seem to the landsman, it has not been lived. It cannot now be lived, and for the outsider to write as if he knew is fatal when those old ships we all still love are concerned. Those who do often remind me that when my brother, a very good seaman, was standing on a Chicago wharf he was approached by a loafer, hoping to engineer a drink, and asked if he was a sailor. When he said he was, this wharf rat remarked, "I'm a bit of a sailor myself." On interrogation it turned out he had not been to sea, had not sailed on the lakes, or even been in Mississippi steam-boats. "Then what in thunder did you do?" asked my brother. "Well, you see, it's this way," said this bit of a sailor himself, "I—I druv mules on the Wabash Canal." There are not a few writers of that order about now. However, I own that there are some sound enough stories written by a few men, and they are very few, who were, as they say, nowadays, "in sail." And there is a growing literature of the steamship. One engineer at least is writing tales of the sea that may please us. But can anything compensate us for the loss of the real "ship"?

For nowadays the old captains and the ships have departed, and with them we have lost so much, even the beautiful old sea-language which no study of student sea-writers can restore. But even with steamships there must still be the terror of the seas to encounter, the heavy gales, the fogs and ice, and all those accidents which bring tragedy and its pain and pity and purification to our hearts. For have I not spoken of Conrad's *Typhoon*, where the battered old tramp-steamer emerged from the windy hell of a cyclone in the Eastern seas? Yet we may well sigh for the past, the past that is no more. Consider that in the last six years I have been over

60,000 miles in steamers and have seen—how many sailing ships? Not one, nothing but four-masted or five-masted schooners, pure modern inventions without real beauty. We shall see no more going about their business in "tall waters" such ships as the *Cutty Sark*, though I rejoice to know she yet lives redeemed from alien crews, or the wonderful *Thermopylæ*, or the *Fiery Cross* or *Flying Cloud*. Many of these I have known, but only the old *Cutty Sark* remains, and she will not again do the work of the seas. Never again shall I or any of us, it may be, come up with and pass, or perhaps be passed by, a full-rigged ship driving before the gale when she is running down her easting in the Roaring Forties. To see her lift and pitch and drive and thrust out her fore-foot forty feet under the grey-arched curve of her reefed fore-sail and topsails, to see the very scud of the skies down upon her stowed royals, as the grey-green seas are white with rolling spindrift, the foam of driven and tormented waters; to lift one's hand in greeting to the few upon her poop as we pass by—yes, what chance of this again? So it is well that some live and lively records of the old ancient sea from which we spring should be left to us. We find much in the grave and simple stories of the old navigators. Sometimes their very simplicity reaches strange heights of tragedy. But in their constant use of the sea they took its passions and perils for granted, and told us as little as they might, not knowing we should hunger to learn what they felt and what they thought. This simplicity, this reticence, this sparing employment of detail must still mark the best work of the sea, as it marks all literature. We shall, I think, find it in all stories written by men who can tell us just the little more we wish to hear of those who went down to the sea in the ships which now lie, as it were, at anchor in the ports of memory, with their bleached canvas harbour-furled.

[M. R.]

WEEKLY EVENING MEETING,

Friday, May 25, 1928.

Sir Robert Robertson, K.B.E. M.A. F.R.S.,
Honorary Secretary and Vice-President, in the Chair.

Alfred C. Egerton, M.A. F.R.S. M.R.I.,
Reader in Thermodynamics, University of Oxford.

Engine Knock and Related Problems.

One Hundred and Thirteen years ago Sir Humphry Davy commenced his magnificent researches on Flame, which paved the way for all subsequent work on Combustion. It is interesting to remember that Davy specifically thanked Michael Faraday, then twenty-four years old, for "his able assistance" during that work.* It was the practical aim to combat the dangers of firedamp in mines that supplied the incentive to Davy's work on combustion. To-day practical ends still supply the main incentive to research on combustion. In one way or another improvement of the internal combustion engine is the source of much of the work that is done.

Whether we like or like not the advent of the internal combustion engine and the changes it has wrought, no one can deny the human achievement it represents. It is only necessary to recall the nature of the cycle of operations, the high temperatures involved, the exact timing of each function, and the speed with which those functions have to be carried out.†

Amongst the noises which this product of human enlightenment has bequeathed to us, we have what is called engine "knock" or "pinking." "Knocking" is a sound which comes from the cylinder during the abnormal explosion of the charge. It is to be distinguished from "pre-ignition"—usually a duller sound—which is caused by explosion of the charge prior to ignition by the spark, and which may under certain circumstances also arise. "Knocking" limits the compression of the charge. The following figures will make clear the effect of compression of the charge on the efficiency of the engine:—

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* A flame experiment was exhibited (see Chem. News, 105, 232, 1912), which drew attention to the effect of small quantities of one combustible substance on the combustion of other mixtures. The importance of such effects was first established by Davy, and they form the main theme of the present discourse.

† A slide showing the four strokes of an internal combustion engine was exhibited, attention being drawn particularly to the temperature, about 450° C., at the end of the compression stroke.

Compression Ratio.	Theoretical Efficiency.	Gain of Efficiency.
4 : 1	42·6	—
5 : 1	47·5	11·0 per cent.
6 : 1	51·2	8·0 ,,
7 : 1	54·0	5·5 ,,
8 : 1	56·5	4·5 ,,

If the compression ratio were raised from 4 to 6 there would be a gain in efficiency of nearly 20 per cent. The efficiencies of actual engines are considerably less, something of the order of 30 per cent. at 4 : 1, and 35 per cent. at 6 : 1, or a gain of about 16 per cent. in efficiency. Mr. Ricardo, whose work has so greatly influenced engine design, is of opinion that for engines of, say, 3 inches to 4 inches cylinder diameter the maximum practical efficiency would be given by a compression ratio of about 7 : 1, taking into account the mechanical forces developed, and the means adopted to withstand them. It is not possible to reach such pressures, because the behaviour of the petrol fuel limits the pressure. In an ordinary automobile engine compression beyond about 4-5 : 1 leads to "knocking," and consequent loss of power and damage. By careful design it is possible to arrive at compression ratios of 5 or even 6 : 1, but only at the sacrifice to some extent of other convenience. This forced limitation of the efficiency of the engine is unfortunate as applying to the automobile, but still more so to the aeroplane.

The world's production of petrol is of the order 12,000 million gallons per year. A gain of 20 per cent. on the efficiency might save over 2000 million gallons of petrol. Any opinion as to the reserves of petroleum is not to be hazarded, but it is sufficient to say that any factor which could save such vast quantities of valuable raw material must be very potent economically. ("Cracking" and recovery of light fractions has increased the quantity and improved the quality of the petrol obtainable from petroleum, but the improvement as to compression attained is insufficient.)

It has long been known that benzene, toluene, xylene, etc., added to petrol improve its qualities as regards "knocking"—but to raise the compression ratio from 4·5 to 6 : 1 would require the mixture to contain about 50 per cent. benzene by volume. The world's production of benzene, limited as it is by the production of coke, is only a fraction of that which would be necessary to add to petrol to make it possible to use, universally, high-compression engines. In an effort to find something more effective than benzene, Messrs. Midgley and Boyd, of the Research Department, General Motors, Ltd., tried many thousands of different chemicals and came across lead tetraethyl. Since, in a detonating explosion, the luminous and ultra-violet radiation is more intense, they thought that a substance absorbing radiation might possibly affect knocking. They were led, therefore, to test iodine; it was found to be effective, but not for the reason which suggested its trial, for bromine was found ineffective and chlorine even to induce knocking. They tried another neighbouring group of elements—tellurium, selenium and sulphur finding a similar relationship. Then proceeding to try compounds in other groups of elements, which were soluble in petrol such as tin and lead ethyls, they discovered the remarkable potency as an "anti-knock" of the latter compound.

Many other substances, like diethyl telluride, or the unstable metallic carbonyls, behave similarly to a more or less marked extent. There are also organic products, such as the aromatic amines, which are effective, but about 40 times as much would need to be added for the same effect as would be obtained with lead ethyl. It is not really satisfactory to give figures of effectiveness, as that seems to vary according to the circumstances of the test. The curve gives figures for the effect of small volume percentages of lead ethyl on the highest compression reached before knocking becomes audible, using a Ricardo variable compression engine (see Fig. 1). In Ricardo's engine the compression is varied by raising or lowering the whole cylinder, carburettor, camshaft and valve gear, the movement being measured by a micrometer screw. The engine is coupled direct to a balanced swinging field electric dynamometer. Another method of estimating the "knocking" characteristics of a fuel is provided by the fixed compression Delco engine, made by General Motors, Ltd. The extent of knocking is measured by a bouncing pin arrangement, the extent to which it is bounced being measured by the volume of hydrogen generated during the time the pin closes an electrical circuit. The "knocking" character of a fuel can be compared against a standard fuel containing a definite quantity of anti-knock. One part of lead tetraethyl in 1500 parts of petrol (about 5 c.c. of ethyl fluid per gallon) will permit of the use of a compression ratio up to 6 : 1, and give an increase of at least 10 per cent. in the power thereby. Furthermore, if all petrol were to be treated with such a "dope," it would be quite feasible from the point of view of supply, because it would entail only about 40,000 tons of lead per annum, which is not more than 2 per cent. of the world's production of that metal. It is not the purpose of this discourse to enter into any controversial matters as to the poisonous character of ethyl lead or its effect on the engine. It is more interesting to consider "why anti-knocks anti-knock?" Why should it be particularly interesting?

The two remarkable points are the highly inflammable nature of the compounds which act as "anti-knocks," and the extraordinary small quantities required to be effective: 1 molecule of lead in 200,000 molecules of fuel-air mixture is definitely effective.*

The Asiatic Petroleum Company afforded me facilities for investigating the problem, and I propose to outline the work as it developed from one stage to another. Discussion with Mr. H. T. Tizard, F.R.S., led to the first line of attack. It was assumed that "knocking" in an engine—engine detonation—was akin to the setting-up of detonation in a gaseous mixture in a tube—a supposition later abandoned. The plan was to get an explosive mixture to detonate in a tube at a definite place, and then to find how the position would be altered by the presence of anti-knock. If the anti-knock delayed combustion, then the position of detonation

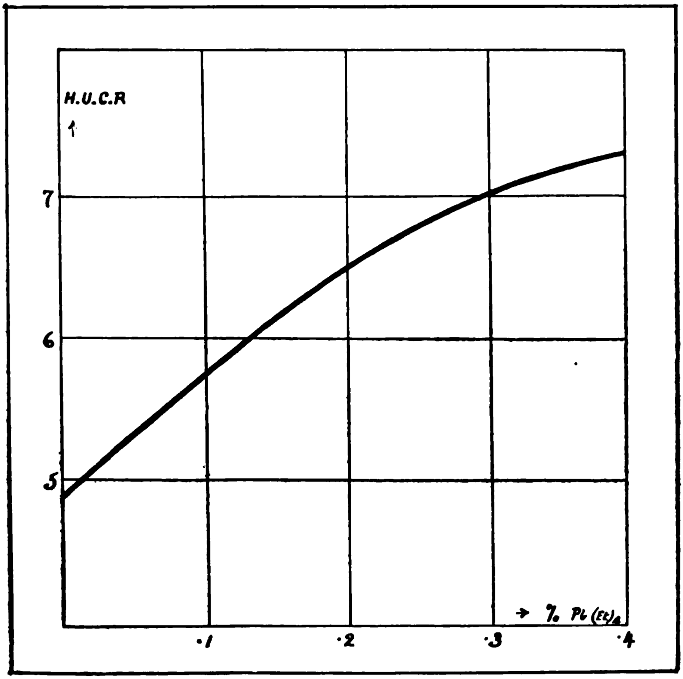

Fig. 1.

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* The highly inflammable character of iron carbonyl was illustrated, and the high explosibility of lead tetraethyl when heated under pressure.

ought to be further along the tube. Le Chatelier pointed out long ago the various ways in which flame can be propagated in an explosive mixture of gases. Depending on the conditions and the constitution of the mixture, flame may either be propagated with a uniform velocity by conduction of heat from layer to layer, or a vibrating type of combustion may be set up, or the flame may uniformly accelerate and finally detonate, if the strength of the mixture is above a certain limiting concentration. For the development of a detonation wave the gas in front of the explosion front must be heated by compression at least to its ignition temperature, and the rate of reaction must be so great that the fuel molecules are burnt before ever the compression wave has passed ; combustion and compression then proceed simultaneously and with constant velocity. For instance, the rate of travel of the detonation wave in an acetylene 1.5 oxygen mixture at normal pressure is about 2,700 metres sec. The photographic method, as developed by Prof. H. B. Dixon, was used in the investigations which Mr. Gates and I carried out on acetylene, pentane and hydrogen mixtures with various diluent gases.*

If the mixture is fired at the open end of a tube the record of the travel of the explosion is very wobbly and detonation may occur at almost any place, but if the mixture is fired near the closed end of the tube the explosion will be uniformly accelerated until detonation sets in, and it was found possible for given conditions (as to size of tube, etc.) to obtain detonation at a definite distance from the spark, and to study the effects of different diluent gases on the position of detonation.

Mixture + Diluent.	Position of Detonation.	Mixture + Diluent.	Position of Detonation.
C_2H_2 2·5O_2		C_5H_{12} 8O_2	
	cm.		cm.
3O_2	30	2O_2	12
3A	35	2A	34
3N_2	48	2N_2	50
3CO_2	98	2CO_2	62
3C_2H_2	53	2C_5H_{12}	80

Tube 0·9 cm. diameter. Pressure 760 mm.

It was found that anti-knocks did not influence the position of detonation (except in one or two cases when the influence was to render the position earlier rather than later). These results have been confirmed by Lafitte working in Paris.* At this stage a rather surprising negative result was all that had been reaped. It was necessary to go to conditions a little more like those in the engine—higher initial temperatures and higher initial pressures. For that purpose a long tube with a number of small glass windows was constructed; it could be heated electrically, and could withstand considerable pressures. It was found that for a given mixture increase of pressure diminished the distance from the spark at which detonation is set up, up to a certain limiting pressure, further increase having then very little effect.† (A similar effect of pressure on the *velocity* of the detonation wave is known from Prof. Dixon's work, and was confirmed.) Again it was found that although initial pressure up to seven atmospheres and initial temperatures of 230° were reached, there was no effect of anti-knocks (such as lead ethyl or nickel carbonyl) on the combustion of such detonating mixtures.

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* (See Proc. Roy. Soc., A vol. 114, 137 and 157, and vol. 116, 516, 1927).

Fortunately another line of attack was now open. Dr. Weerman found that the igniting temperature of petrol when dropped into a heated iron pot through which air was gently blown was very considerably raised by the presence of anti-knocks. It was a curious result, for others had stated that there was no such effect of "anti-knocks" on self-igniting temperatures—the difference was merely that air was used by Dr. Weerman instead of oxygen. We confirmed Dr. Weerman's results, and used the method to elucidate what was the seat of action of anti-knocks on the processes of combustion.

	Rise of Igniting Temperature.
	°C.
Selenium Diethyl	140
Iron Carbonyl	130
Bismuth Triethyl	120
Lead Ethyl	90
Nickel Carbonyl	40
Tellurium Diethyl	55
Bismuth Triphenyl	42

First, from a spark between lead electrodes, lead oxide was led into the igniting pot along with the air. (See Fig. 2.) It was ineffective, as is explained further on. Then, lead was brought in as metallic vapour from an arc in an argon stream, with immediate success—the igniting temperature was considerably raised, and to much the same extent for a given amount of lead so introduced as when introduced as lead tetraethyl as solution in petrol. So the lead part of the molecule dissociated from the ethyl molecule was essentially the active constituent. This being possible with lead, other volatile metals could be tested (see Table I.), and so were traced the ignition inhibiting properties of many elements. Thallium was found the most effective.* Potassium was curiously very effective—an illuminating fact. The potassium vapour, as soon as it meets the air stream, must become oxidised, so it must have been the oxide which was effective. But if it is brought into the air stream before reaching the pot it is not effective. The explanation to this paradox seems definitely to be that the oxide is effective if it is there in a molecular condition, but if it has time to conglomerate into

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* A demonstration of the detonation of an acetylene mixture was given; also a slow motion slide of a detonating explosion.

† See Proc. Roy. Soc. A 114, 152, 1927.

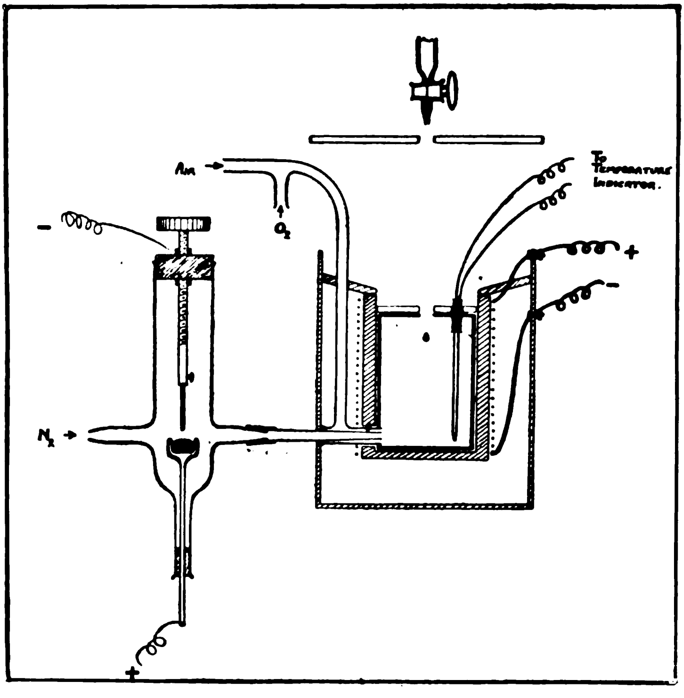

Fig. 2.

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* The effect of Thallium vapour from an arc in argon in raising the ignition temperature of petrol about 100° was illustrated. The projection temperature indicator was kindly lent by the Cambridge Instrument Company, Ltd.

"chunks" of oxide, it becomes practically ineffective. Many facts support this hypothesis. Following on this work, Mr. Ricardo and Mr. Thornycroft ran an engine with an arc attachment, and found that, even if air instead of nitrogen was passed over the lead arc, it was still quite effective. In that case there was, no doubt, plenty of lead oxide in molecular condition, whereas in the preliminary experiment which we described with the spark in air which failed to give an effect the lead oxide was probably already conglomerated.

TABLE 1.

Effective.	Ineffective.	Doubtful.
Thallium	Aluminium	Tin*
Potassium	Magnesium	Cerium*
Lead	Mercury	Vanadium*
(Iron)*	Iodine	Titanium*
(Nickel)*	Phosphorus	Zirconium
Manganese	Gold	Thorium*
Bismuth	Zinc	Tantalum
Selenium		Tungsten*
Tellurium		Chromium*
Sodium		Cobalt*
Cadmium		Uranium*
Calcium		
Antimony		

* Tested by other methods.

Summarising results up to this stage, it is found :—

(1) That anti-knocks do not affect a rapidly accelerating explosion in a tube.

(2) That anti-knocks influence igniting temperatures of petrol (in a current of air).

(3) That in every case an anti-knock effective in the engine influences igniting temperatures, though in some cases the reverse is not found.

(4) That it is the metal part of an organo-metallic anti-knock which is mainly instrumental in the action.

(5) That this metal atom must be in an incipient state of oxidation.

Although there are now some experimental facts to which "to appeal," there are many questions to answer. Is there any property in common between the substances which have effect? What is the function of the anti-knock when it raises the temperature of ignition in the igniting vessel? In what stage of affairs in the engine does the anti-knock have effect, and why, if it doesn't influence the rate of a detonating explosion, does it influence combustion in the engine cylinder?

As to the first question, those substances, it seems, which act as anti-knocks have the common property that a state of equilibrium exists at the temperature at which they operate between certain products of the anti-knock. Thus, in the case of potassium, the oxides K_2O_3 and K_2O_4 have been shown to exist in a state of equilibrium in favour of the higher oxide at 400°. If the higher oxide is reduced to the lower, it can be regenerated by the next suitable impact with an oxygen molecule. It seems to be this property which renders such a small amount of metal effective ; as soon as some has had effect it is regenerated in an active condition.

Similarly with thallium, lead, bismuth and manganese, such figures as these are for the oxide equilibria support the hypothesis. The metals zinc, magnesium, etc., are ineffective, for no such transformation is possible. The case of selenium is interesting, the same sort of change may take place as occurs with sulphur compounds such as glutathiones in physiological oxidations. Some organic substances such as aromatic amines act as anti-knocks, though to a feeble extent compared with metallic compounds, their behaviour also agrees with the suggestion that their activity is due to the formation of fairly stable oxidation products. In support of this we find quinone to be effective. These organic anti-knocks were shown to undergo combustion themselves at the temperature they are required to operate, so that the chance of their effective action is very much reduced.

The next question was what do the anti-knocks do to the vapours which would ignite ? A study of the behaviour of a large number of inflammable substances in the igniting pot and the effect of anti-knocks of various kinds upon them has been made.* Anti-knocks definitely slow down the rate of reaction prior to ignition, as curves showing the temperature rise indicate. This result was confirmed by measurements of CO_2 produced, and by Dr. Weerman of oxygen absorbed. In the language of the chemist the anti-knock is an "inhibitor of oxidation." Then comes the question, is it in the body of the gas or on the surface of the vessel that the anti-knock has its effect ? Davy demonstrated that surface combustion occurs under circumstances where flame cannot be set up. From a variety of observations we have come to the conclusion that ignition starts, not at the surface, but in centres of high energy in the body of the gas. The anti-knock delays combustion not specially at the surface, but by inhibiting the rate of oxidation prevents the setting up of these centres. If the surface / volume ratio of the ignition vessel is increased, the effect is to raise ignition temperature. If the anti-knock were to increase the activity of the surface, the effect indeed might be to raise igniting temperature ; but it was not found that the effect of the anti-knock was proportional to the extent of surface.

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* See J. Inst. Petroleum, Tech. 13, 61, 244, 1927.

If the effect of an anti-knock on a range of compounds is studied (see Table 2), it may be noted that the combustion of normal paraffin hydrocarbons (which are those constituents of petrol that are most prone to knocking "), is more inhibited than that of naphthenes, for instance, such as cyclohexane. Further, there is a very remarkable inhibition of the combustion of aldehydes. Professor Bone showed long ago that aldehydes are intermediate products in the combustion of hydrocarbons. One would be led to suppose that this is the seat of the action of anti-knocks, the further oxidation of the aldehyde is delayed. That must be so, but it is not the whole story. Let us assume that it is so, that the anti-knock or inhibitor acts only on the aldehydes in the course of the hydrocarbon oxidation and delays their oxidation. The amount of the aldehyde will then increase in the products of combustion. That is the opposite to what is found.

TABLE 2.

Substance.	I.T.	Rise with $PbEt_4$	Substance.	I.T.	Rise with $PbEt_4$
	°C.	°C.			
Benzene	690	18	Acetaldehyde (iron vessel)	395	110
Cyclohexane	535	27	do. (silica vessel)	350	170
Methylcyclohexane ...	470	92	Pentane	540	87
Isohexane	525	46	Amylalcohol ...	490	Nil
Heptane	430	83	Amylenehydrate ...	640	35
Petrol (Shell) ...	460	82	Valericaldehyde ...	320	390
Ether	440	65	Dimethylaniline ...	445	Nil
Alcohol	515	(9)	Carbon Bisulphide	120	Nil

Therefore to retain our hypothesis it could be suggested that the products of oxidation of the aldehydes hasten the initial hydrocarbon oxidation—a process of "autoxidation." The consequence of such an addition to the hypothesis would be that considerably enhanced combustion should occur on addition of aldehydes to hydrocarbons. That is also not found to be the case to any marked extent. We have then to discard the hypothesis.* The inhibitor must be acting elsewhere besides. Suppose, then, that it also inhibits the initial oxidation of the hydrocarbon. Here, again, there is a difficulty. The quantity of inhibitor is insufficient to control the simple interaction of oxygen and hydrocarbon molecules. It must then be concluded that it acts on certain products of the primary oxidation of the hydrocarbon which tend to hasten ("autocatalyse") that initial oxidation. Such is the argument by which we are guided to a general view of the process of combustion of a hydrocarbon.

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* The reason for this lack of effect is probably that the concentration of the aldehyde is not great enough to transmit the reaction chain.

Suppose some fuel molecules are amongst oxygen and nitrogen molecules in a vessel, the temperature of which is gradually rising. The molecules primarily pick up their energy from the walls. It is the walls that control affairs at first, and reaction only occurs at the walls at first. Sufficient of the fuel molecules in the body of the gas may, however, gain momentarily sufficient energy to unite with an energetic oxygen molecule with which they happen to encounter.* That means essentially that a temporary peroxide is formed. A peroxide in a high energy state possessing the energy of activation of the fuel and oxygen and also potentially the reaction energy. Now, several things may happen to this active addition compound of fuel and oxygen.† (*a*) It may revert to a normal state and radiate energy in so doing, and form a stable peroxide. Peroxides are indeed found amongst products of combustion and of oxidation of organic compounds (as Wartenburg, Staudinger, Callendar and Mardles and others have shown). (*b*) The compound may break up again, the parts being less active by the energy radiated. (*c*) The compound may reorganise and give highly active products, thus for ethane: $CH_3-CH_3+O_2 \rightarrow CH_3.CH_3.O_2 \rightarrow CH_3CHO+H_2O$ (Dihydroxyethane may possibly also be formed to some extent as an intermediate stage in the production of the aldehyde and water); or (*d*) collision may occur. Encounter with inactive nitrogen molecules may result merely in frittering down the energy of the active product, but encounter with a fairly active fuel molecule, or product thereof, or oxygen molecule, will raise its energy so that it will react and produce active products. These products in turn may collide and activate other fuel molecules, and so on, a reaction chain mechanism being set up. In the above case of ethane, $CH_3-CH_3.O_2 \rightarrow CH_3CHO+H_2O$, the active aldehyde and/or water molecules being able to communicate their energy to activate other ethane molecules, and so on. A rare occurrence thus becomes a frequent occurrence locally. This explains the setting up of centres of high energy in the body of the gas, indicated as necessary for ignition to occur. Any influence that tends to decrease the local concentration either of fuel or oxygen molecules or to dissipate the energy may break the chain of reaction, and so enormously affect the rate of oxidation. Once a chain mechanism is established local free energy increases, and molecules can be excited into higher energy states and in returning from those states can give out light. So it is that luminescence can be observed during combustion of hydrocarbons many degrees below their igniting temperatures. Further, as the mean temperature approaches the igniting temperature, the state of excitation becomes sufficiently intense to ionise a proportion of the molecules, actual inflammation being attended with more or less intense ionisation. Some sort of solidity is derived in support of the view put forward in that Dr. Bäckström,* working in Sweden, has found that the thermal oxidation of liquid aldehydes is essentially a chain reaction. More important still, Mr. Hinshelwood can best explain his recent measurements of the rate of reaction of oxygen and hydrogen in terms of a reaction chain mechanism. The effect of the surface of the vessel in breaking the chains is clearly indicated, and seems to be in accord with the observation before mentioned that increase of surface raises igniting temperature.

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* Prof. Callendar and his associates have investigated this temperature of initial combustion.

† Dalton's method of depicting atoms with their "heat atmospheres" was employed to illustrate the behaviour of these "active" molecules.

Returning now to the "anti-knocks," it is established that they inhibit oxidation. They evidently act by breaking the chains. That, together with the chance of regeneration already mentioned, is sufficient to account for the small quantity of the "dope" required. Probably they break the chain because reaction between metal peroxide in a molecular condition and active fuel peroxide occurs with mutual destruction and rearrangement, the resulting products having much less energy than they would possess had the fuel peroxide encountered another active molecule—the reaction, for instance, $H_2 + H_2O_2 \rightarrow 2H_2O$ liberates much more energy than the reaction $H_2O_2 + PbO_2 \rightarrow PbO + H_2O + O_2$, so that the H_2O produced would be much less active in the latter case, and could not communicate sufficient energy to continue the reaction chain. They are not effective in presence of high concentration of oxygen, for then the reaction chains are probably too easily propagated for their inhibition. Catalysts (such as nitrogen or chlorine peroxides) promote autoxidation by providing highly active products on reaction.

This question of the behaviour of "anti-knocks" has led deep into the processes of combustion; enough has been said to indicate that much more work must be done before the detail is known to any degree of certainty.

Finally, we come to the third question, "In what stage of affairs in the engine do the anti-knocks have effect, and why, if they do not influence the rate of a detonating explosion, do they influence combustion in the engine cylinder?"

When it had been shown that anti-knocks delay oxidation, Mr. Ricardo ingeniously arranged a test whereby it was found that preliminary oxidation during the compression stroke was much less when inhibitors were present than when they were not present. (This conclusion was likewise arrived at by Professor Callendar and the Air Ministry staff.) It was clear that anti-knocks act on the gaseous charge in the stage prior to ignition (and prior to inflammation at the flame front), and delay the initial stages of reaction in the manner already discussed.

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* Cf. the important experiments of Moureu and Dufraisse on autoxidants.

Now, in front of a flame travelling along a tube there is a narrow region—a fraction of a millimetre in thickness—in which the gas is becoming heated to such a temperature that reaction is rapid enough for inflammation to be set up. Such a temperature is the ignition point under the circumstances in which the gaseous mixture there finds itself. There is a further rapid rise of temperature within the flame front due to the combustion of the mixture. If one knew all about the reaction velocity, the thermal conductivities, specific heats, and ignition point, under given conditions of loss of heat, it ought to be possible to calculate the flame velocity. Such data are not complete, but it can be realised at once that any influence which delays matters in the "heating up" zone will influence the rate. Furthermore, since the zone is exceedingly narrow, it is clear that any inhibitor "will have to be quick about it" if it is going to have any effect, particularly if it has to be first decomposed and rendered in an active condition. When once in the zone of combustion there is so much heat available that the inhibitor can have very little effect—in fact, the organic radicals which form part of the molecule go to add to the heat developed by their own combustion, and tend even to hasten matters; so it is that one can explain the negative results obtained in the detonation experiments, and the absence of effect when gases are suddenly ignited from the cold by adiabatic compression. Some experiments have been made to test this conception. Comparatively slow flames in pentane mixtures of concentration similar to that used in engines when photographed in a bomb were shown to be slower when lead ethyl was present.* The uniform speed of flames in tubes could also be modified to some extent with pentane mixtures. These effects were particularly marked for vibratory flames, for then as the flame moves forward it decomposes and oxidises the lead compound, and on its return conditions are more favourable for the lead to be effective as an inhibitor. Using carbon monoxide or methane flames, no effect was observed with lead ethyl, but with the more easily decomposed iron carbonyl the rate is considerably modified. A 50 : 50 air CO (wet) mixture will burn with a blue flame at a rate of about a yard a second, exactly the same mixture containing $\frac{1}{1000}$th part by volume of iron carbonyl burns with a brightly luminous flame very much slower. (This effect was demonstrated.)

Photographs have been taken of explosions in acetylene and in pentane mixtures at about engine strength in a cylindrical bomb (19 cms. × 10 cms.) fitted with three windows, ignition being started in the centre of one of the endplates.* When audible knock occurred, the photographs showed a check in the rate of combustion after the second window. Professor Wheeler points out that this is probably due to cooling of the flame front as it meets the walls. Vibrations in the flame are also visible. Dr. Fenning, at the National Physical Laboratory, has recorded the pressure effects in explosions of a knocking character in various mixtures at various temperatures in a similar bomb. Professor Wheeler and Dr. Maxwell have also taken a number of beautiful photographs of this kind.* Their results indicate that, in a knocking type of explosion, a sudden vibratory and enhanced combustion occurs in the neighbourhood of the walls, which may lead to a compression or shock wave passing back through the products of combustion. It is not a "detonation wave." (A similar effect in a vibratory explosion in coal gas and air at ordinary pressure was shown.) It is "like a great flame which creates noises within the air" (Leonardo da Vinci). "Knocking" appears to be due to inequality in the condition of the charge set up, particularly in regions of high pressure and temperature, as in the neighbourhood of hot exhaust valves. This inequality provides regions of high energy, containing molecules in high energy states, where reaction can spread quicker. Unequal burning gives rise to a vibratory condition of flame. Any influences, such as a higher state of turbulence or cooler surfaces, or more even and longer combustion space, which tend to prevent sudden aud local rise of pressure, and the setting up of centres of high energy tend to prevent knocking. "Anti-knocks," such as lead ethyl, by inhibiting the processes of combustion which we have seen to occur in those centres, are therefore effective in preventing knocking. Furthermore, they have been rendered effective by the temperature and oxidation to which they and the charge are exposed during the compression stroke.

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* Proc. Roy. Soc., A 116, 516, 1927.

Professor Callendar has drawn attention to the importance of the presence of nuclei—small droplets of unevaporated fuel—in the charge in the engine cylinder. A discussion of this aspect of the subject cannot be here entered upon. The concentration and energy of the molecule of vapour at the surface of an evaporating droplet, and the rate of oxidation at the surface of the droplet, are such as greatly reduce the temperature of ignition. The tendency to knock would likewise be enhanced. But it is also certain that knocking is possible in completely vaporised mixtures, and that anti-knocks will affect the temperature of ignition of completely gaseous fuels. A more general standpoint is adopted in this evening's discussion of engine-knock and the problems that relate thereto.

We have come now to the end of our story—not though to the end of an investigation. Thinking again of Davy's researches on Flame—of the questions, how does a flame start? what prevents it starting? what occurs at the surfaces exposed to the ignitable gases? we see the horizon of the unknown ever widening. We will quote from the opening remarks of Faraday's lectures on the Chemical History of a Candle, "So abundant is the interest that attaches to the subject, so wonderful are the varieties of outlet which it offers into the various departments of philosophy. There is no more open door by which you can enter to the study of natural philosophy than by considering the physical phenomena of a candle."

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* J. Pet. Technologists, Feb. 1928. See also Duchesne, C. R, 186, 220, 1928.

[A. C. E.]

GENERAL MONTHLY MEETING,

Monday, June 4, 1928.

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

The Right Hon. Sir John Maxwell, P.C. G.C.B.
Sydney Walton, C.B.E. M.A. B.Litt.

were elected Members.

The Special Thanks of the Members were returned to the Council of the Institution of Electrical Engineers, for their gift, through Mr. R. W. Paul, M.R.I., of a photograph of the portrait of Michael Faraday, by George Harcourt, R.A.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same:—

FROM

Egerton, A. C., Esq., F.R.S. M.R.I.—Reprints of Papers by Sir H. Davy in the Philosophical Transactions, with corrections by the Author. 4to. 1801–1812.

Liverpool, Corporation of, Library Committee—Liverpool Prints and Documents Catalogue. 4to. 1908.

Lloyds, The Committee of—A History of Lloyds. By C. Wright and C. E. Fayle. 8vo. 1928.

WEEKLY EVENING MEETING,

Friday, June 8, 1928.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Hon. Secretary and Vice-President, in the Chair.

GEORGE P. THOMSON, M.A.,
Professor of Natural Philosophy, University of Aberdeen.

The Waves of an Electron.

THE history of the newer physics, as it may be called, is largely the history of the very successful attempts which have been made to explain the facts of the physical universe in terms of electricity. In particular, the natural units of electricity, the electron first isolated as the carrier of the negative charge in the cathode rays, and the more massive proton with its equal positive charge, have been the bricks out of which physicists have tried to build model atoms. But until recently the only properties which could be attributed with certainty to the electron were its mass, 9×10^{-28} gram., and its charge, $-4 \cdot 77 \times 10^{-10}$ electrostatic units. It is indeed possible to calculate a "size" on the assumption that it follows the same laws as a charged metal sphere in a laboratory, but the value found, $3 \cdot 7 \times 10^{-13}$, is neither confirmed nor contradicted by any experimental evidence.

To show, as I hope to do to-night, why we are now obliged to ascribe much more complicated properties to the electron, I must make a digression into optics, as it was from this direction that the theory comes which lead to the experiments I have to describe. It is well known that there are a number of phenomena which are instances of wave-motion in what used to be called the ether, though it seems now more fashionable to call it space. Such are wireless waves, light, ultra-violet radiation, X-rays. They differ only in wave-length, and it is convenient to refer to them all as light.

Now at the beginning of the century the wave theory of light was one of the most firmly established parts of physics. It is true that for many purposes light was treated as consisting of rays, i.e., effects travelling in a straight line, but this was merely an approximation used for convenience in calculation, and not regarded as of any great physical significance. The evidence for waves (interference and diffraction) was, and remains, overwhelming. Gradually, however, a series of experimental and theoretical results accumulated which showed that this was not the whole story, and in particular that light could communicate an amount of energy to an electron which only depended on the *kind* of light and not at all on the *intensity*. If light consists of continuous waves this seems almost impossible; but it is just what we should expect if, as Newton firmly held, light is due to a stream of corpuscles. In such a case if one of these hit an electron it would give to it an energy which would be quite independent of the number of other corpuscles present in the stream, i.e., of the intensity of the light.

Prince Louis de Broglie has hit on a brilliant synthesis of the two views. Put very briefly it comes to saying that light is indeed corpuscular, but that the corpuscles are accompanied and guided by waves. Now the bearing of this on our subject is as follows: de Broglie's theory is quite a general one, and should apply to *any* sort of corpuscular motion, in particular electrons should be accompanied by waves which determine their motion. It would seem at first sight that this would involve a contradiction with what is already known about the motion of electrons. For example, waves spread after passing through a small aperture, while electrons can be concentrated into a fine beam by a tube or series of slits. But it is all a question of wave-length. Ordinary visible light only spreads very slightly round obstacles because its wave-length is small compared with the size of ordinary objects. Wireless waves bend much because their wave-length is large.

There is in fact a relation pointed out more than a century ago by Hamilton between optics and dynamics. The laws of dynamics in their most general expression, the principle of action, are mathematically identical with Fermat's principle which is the most general statement of optics on the ray theory. Now in optics rays are an approximation to the wave theory, more and more nearly true the smaller the wave-length; perhaps the accepted mechanics of Newton and Hamilton is also only an approximation, true in practice because the wave-lengths concerned are very small? If so, we must develop a new mechanics to deal with cases when the wave-length is not of negligible size.

To-night I shall only speak of the mechanics of a free electron. Earlier in the year the Institution had the privilege of hearing from Professor Schrödinger an account of the rapid progress that has been made with the wave-mechanics of the electrons inside the atom, a progress mostly due to Professor Schrödinger himself.

De Broglie's results are as follows :—For any moving particle the wave-length $\lambda = h/$ (momentum) where h is Planck's constant $= 6.58 \times 10^{-27}$. This in fact comes out negligibly small for any ordinary body. Even for an electron moving with the moderate energy of 150 volts it is 10^{-8} cm., or about that of a soft X-ray while for cathode rays of, say, 15,000 volts it is 10^{-9} cm.

Still, Sir W. Bragg and his son have shown us how to measure the wave lengths of X-rays, small though they are, and the electron waves may yield to a similar treatment.

We must first consider some general properties of these waves. According to de Broglie the speed of the waves, V, is c^2/v, where v is the speed of the particle, and c the velocity of light. They are thus faster than light. This result would be contrary to relativity if the waves carried energy, and so could transmit a signal, but as the energy is supposed associated only with the *particle* this objection does not come in ; indeed, on the classical optical theory, cases are known of metals, such as sodium, having a refractive index less than unity, which implies a wave velocity greater than c. From the above two equations, taking account of the variation of mass with speed, $V^2 = c^2 + \frac{m_0{}^2 \lambda^2}{h^2 c^4}$, and so depends on λ. Thus there is *dispersion*, in the optical sense, for these electron waves even in free space.

The late Lord Rayleigh showed that in such a case, besides the velocity of the independent waves, there is what he called a *group velocity*, which is the speed with which the pattern formed by the superposition of several waves of slightly different wave-length moves along. Now an application of his formula shows that in this case the group velocity is precisely v, the speed of the electron, so that the electron carries its *group* with it (or the group carries the electron, if you prefer it), though the individual waves composing the group are perpetually shooting through from behind.

Again, since an electron is charged, its motion is affected by an electric or a magnetic field. On the older view of an electron this was simply a consequence of the laws of electricity. On the new view the electron goes where its wave takes it. Somehow the only possible paths are those along which the wave is propagated. On this view the change in motion of the electron is analogous to the change in direction of a ray of light when it enters a refracting medium. In fact, by choosing the field in an appropriate manner, we can imitate the effect on light of a corresponding piece of glass. For example, we can make a field equivalent to an ordinary convergent lens and bring electrons to a focus.

But, of course, this is only a translation between one view and the other. To test the new theory we must use something which has a regular structure comparable in size with the supposed wave-length. Here the wide experience obtained with X-rays comes to our aid, and the natural regularities of a crystal give what is needed. The regularly spaced atoms of a crystal each scatter the waves, since they scatter the electrons. Such a system of regularly arranged scattering points produces, as is well known, a peculiar effect on a simple wave, which is deflected into a few definite directions. It forms, in fact the three dimensional analogue of the ordinary optical ruled diffraction grating. The effect can be illustrated in its simplest form with waves on a liquid.

The idea of using a crystal in this way has been carried out by Davisson and Germer in America, and by Reid and myself in this country. As my time is limited, you will, perhaps, pardon me if I limit my description to our work, especially as Davisson's and Germer's experiments lead to the same general conclusions. As the atoms of a crystal are arranged in three dimensions, the result is somewhat more complicated than in the experiment you have just seen. If a beam of X-rays is passed through a single crystal and allowed to fall on a photographic plate, the developed picture is a series of spots whose position depends on the structure of the crystal. If it passes through a powder formed of a large number of very small crystals arranged at random the spots due to each tiny crystal join together to form a series of concentric rings, whose relative sizes depend on the crystal structure, the absolute size of the whole pattern being proportional to the wave-length of the X-rays. Both these methods, or their equivalent, have of course been extensively used in the investigation of crystal structure by X-rays, nowhere more successfully than in this Institution.

The apparatus we have used is a direct application of this method to the study of electrons.

It is necessary to use very thin films because electrons have vastly less penetrating power than X-rays, and they must not only penetrate the film, but be so little affected by it that the scattering is what is technically called "single." In practice this means that only a small minority should be appreciably scattered at all. The films we used were only a few millionths of a cm. thick, in fact it is no use trying a metal film unless it is thin enough to be transparent. Some of the metals show uniform rings, others spots arranged round rings. Both can be paralleled in the analogous results with X-rays.

In comparing theory with experiments we have three separate checks. First, the relative sizes of the rings must conform to the known crystal structure. For the metals I used these go as the square roots of certain integers. In fact, the first few rings should be in the ratio $\sqrt{3} : \sqrt{4} : \sqrt{8} : \sqrt{11} : \sqrt{12} : \sqrt{16} : \sqrt{19} : \sqrt{20}$. The following table shows the extent of the agreement.

RELATIVE DIAMETER OF RINGS.

Metal . .	$\sqrt{3}$	$\sqrt{4}$	$\sqrt{8}$	$\begin{Bmatrix}\sqrt{11}\\ \sqrt{12}\end{Bmatrix}$	$\sqrt{16}$	$\begin{Bmatrix}\sqrt{19}\\ \sqrt{20}\end{Bmatrix}$	$\sqrt{24}$	$\sqrt{27}$	$\begin{Bmatrix}\sqrt{35}\\ \sqrt{36}\end{Bmatrix}$
Gold . .	$\sqrt{2.93}$	$\sqrt{4\cdot02}$	$8\cdot00$	$\sqrt{11\cdot2}$		$\sqrt{19\cdot8}$			
	$\sqrt{3\ 05}$	$\sqrt{4\cdot05}$	$8\cdot00$	$\sqrt{11\cdot4}$		$\sqrt{19.1}$			
	$\sqrt{2\cdot96}$	$\sqrt{4\cdot08}$	$8\cdot00$	$\sqrt{11\cdot2}$		$\sqrt{19\cdot5}$	$\sqrt{22\cdot7}$	$\sqrt{26\cdot6}$	$\sqrt{35\cdot4}$
Aluminium		$\sqrt{4\cdot00}$	$8\cdot00$	$\sqrt{10\cdot9}$	$\sqrt{16\cdot5}$	$\sqrt{20\cdot4}$		$\sqrt{27\cdot0}$	
		$\sqrt{3\cdot98}$	$8\cdot00$	$\sqrt{11\cdot0}$	$\sqrt{15\cdot7}$	$\sqrt{21\cdot2}$		$\sqrt{27\cdot8}$	
		$\sqrt{4\cdot02}$	$8\cdot00$	$\sqrt{10\cdot9}$	$\sqrt{15\cdot9}$	$\sqrt{19\cdot4}$		$\sqrt{26\cdot8}$	$\sqrt{35\cdot0}$
Platinum	$\sqrt{3\cdot00}$	$\sqrt{4\cdot00}$	$8\cdot00$	$\sqrt{10\cdot9}$	$\sqrt{15\cdot3}$	$\sqrt{18\cdot4}$	$\sqrt{24\cdot0}$	$\sqrt{26\cdot5}$	
	$\sqrt{3\cdot07}$	$\sqrt{4\cdot01}$	$8\cdot00$	$\sqrt{11\cdot3}$					
	$\sqrt{3\cdot04}$	$\sqrt{4\cdot03}$	$8\cdot00$	$\sqrt{11\cdot2}$	$\sqrt{16\cdot4}$	$\sqrt{19\cdot0}$	$\sqrt{23\cdot4}$	$\sqrt{27\cdot1}$	$\sqrt{35\cdot0}$

Secondly, if de Broglie's Theory is true the wave-length is inversely as the momentum, i.e., apart from a small relativity correction the product of the diameter of any one ring into the square root of the measured energy of the rays should be constant.

Aluminium.		Gold.		Platinum.	
Voltage.	D $\sqrt{P}$.	Voltage.	D $\sqrt{P}$.	Voltage.	D $\sqrt{P}$.
21,800	(359)	21,000	(344)	18,200	(378)
34,500	385	33,700	377	25,500	395
45,000	385	44,000	377	34,500	420
57,600	395	55,000	379	40,000	401
64,000	385	58,000	376	45,000	413
Means	387		377		407

Celluloid.			
Voltage.	D $\sqrt{P}$.	Voltage.	D $\sqrt{P}$.
9,800	185	23,200	193
11,500	175	30,500	186
16,100	189	36,000	193
16,800	191	42,500	189
21,000	190	50,000	195

Thirdly, the actual size of the rings is calculable from the assumed wave-length, and the known spacing of atoms in the crystal. The best ways of showing the agreement is to work back, calculating the crystal spacing from the experiments and theory, and comparing with that found by X-rays.

SIZE OF CRYSTAL UNIT.

	Aluminium.	Gold.	Platinum.
X-ray measurements . .	$4{\cdot}046 \times 10^{-8}$	$4{\cdot}06 \times 10^{-8}$	$3{\cdot}91 \times 10^{-8}$
Cathode ray measurements	$4{\cdot}06 \times 10^{-8}$ $4{\cdot}00 \times 10^{-8}$	$4{\cdot}18 \times 10^{-8}$	$3{\cdot}88 \times 10^{-8}$ $3{\cdot}89 \times 10^{-8}$

Finally, in a few cases the individual spots can be used. This is when the small area struck by the rays happens to consist wholly or mainly of a single crystal. In such a case we can find the orientation of the crystal from some of the spots and see if the others check.

I think that this agreement may be regarded as establishing the essential truth of the theory.

But there remain great difficulties of interpretation. What are these waves? Are they another name for the electron itself? How many waves in series does an electron have? What is the relation between the electron waves and those of light and X-rays? Some of these questions I should like very briefly to discuss, but I must warn you that we now leave the sure foothold of experiment for the dangerous but fascinating paths traced by the mathematicians among the quicksands of metaphysics. First, as to the length of the train of waves. Here we have a little direct evidence. The sharpness of the rings depends, among other things, on this length. A short train of waves results in blurred rings. If we suppose the whole width of the rings due to this, I calculate from the fact that it is just possible to separate the $\sqrt{11}$ and $\sqrt{12}$ rings that there must be at least 48 ordered waves in the series. Probably there are many more, as several other causes tend to widen the rings. But this may only apply to my particular experimental arrangement. Theoretically it seems probable that an exact knowledge of the speed of an electron implies that it has an infinite wave-train associated with it, just as ideally monochromatic light implies an infinite train of light waves. Since there cannot actually be an infinite wave-train this means that an electron's speed can never be exactly known, which in one sense is not surprising, for all measurement is approximate, and it can be shown that the conditions under which the velocity of an electron could be found with great accuracy are precisely those in which a long train of waves could accompany it. But suppose we have a short train of waves. The velocity is uncertain. Is this because there are really many electrons concerned and they have different velocities? In most of the experiments one could devise there would be, but I don't think this covers all cases.

Imagine a small speck of radio-active material which emits electrons as fast β rays. If you make it small enough the particles may follow each other at intervals of minutes, or days, or years, if you like. It seems impossible to suppose that these electrons have any connection with each other. They come from different atoms at widely different times. One is forced to the conclusion that each must have its own separate wave system. But this wave train must be finite, probably in fact it is rather short, and one reaches the surprising conclusion that the electron *has* no definite velocity, even though it may be moving in force-free space. This raises obvious difficulties about energy which I do not pretend to be able to solve, but the work of Ellis and Worster on Ra.E. seems to show that there is an uncertainty of this kind, which is in fact of about the right amount.

As regards the relation between electron waves and light waves: they are certainly not the same, even when their wave-lengths are equal. They go at different speeds: one is refracted by electric and magnetic fields, while the other is not, their penetrating powers in ordinary matter are widely different. If they are the actual motion of an "ether" it must differ in some way in the two cases.

My father, Sir J. J. Thomson, has shown that the relation between wave-length and velocity of the electron waves is precisely that which holds for light waves in a medium containing free electric charges. The properties of such a medium have recently become of great importance, for the possibility of long distance wireless has been shown to be due to the existence of a region with these properties in the upper atmosphere. It is tempting to regard this agreement as something more than a mere coincidence, and to consider the waves of the electron as waves in an ether modified by the presence of the tubes of force, which since the days of Faraday many physicists have regarded as having a real existence round an electric charge. Thus the electron would provide, not indeed its own ether, but the necessary modification of that ether.

But I should like also to suggest another possibility, though you may perhaps think it rather a cowardly way out of the difficulty.

Personally I see no necessity for there to be any vibration of a material or quasi-material object. The essence of a wave is the transference of a *state* according to certain laws, for example, a "heat wave" crossing the Atlantic. The state may, or may not, be one of motion. On this view the function of the waves is to produce conditions at the electron which oblige the latter to move along the instantaneous position of the wave-normal (in the absence of a magnetic field), and along another, but determinate, direction in a magnetic field. But though it seems possible to regard the motion of the electron as determined by the wave in its immediate neighbourhood, it must never be forgotten that the direction of each part of a wave-front depends on the history of the whole wave-front. Any circumstance that influences one part of a wave influences the whole, and this is of the essence of wave motion. The electron is influenced by conditions at a distance, but indirectly through its waves. Thus in our experiments the electron is influenced not merely by the nearest atoms of the crystal, but by the whole of the crystal which is exposed to the beam. The *width* of the wave-front must be taken as the cross-section of the tube through which the electrons pass.

But although the whole wave-front, in theory, affects the electron, all parts do not affect it equally. Thus, suppose the width of the beam were diminished by gradually reducing the size of the tube, apart from the obvious diminution in intensity, the only effect would be a diminution in resolving power due to the reduction in the number of grating elements. When the resolving power is large a considerable change, such as is produced by halving the number of atoms affected, produces comparatively little change in the pattern of the diffracted rays, i.e., the distribution of scattered electrons.

The main features of the pattern are determined by the first few elements, just as, on the older view, the direction of the scattered electron would be mostly determined by the forces exerted on it by the atoms near which it goes and comparatively little by the more distant ones.

The easiest way of looking at the whole thing seems to be to regard the waves as an expression of the laws of motion. The uniform motion of Newton's first law is replaced by a simple plane wave, and so on. On this view, the electron remains the reality, and this seems right, for after all it is the electron as a particle which is actually detected in any conceivable experiment, and the waves come to bear the same sort of relation to it that Newton's or Einstein's law of gravitation bears to the planets which obey it.

It is remarkable how near this view comes to that proposed by Newton in the case of Light, which as we have seen is analogous, and I can hardly give a better statement of the position, allowing for slight changes in terminology, than by quoting what he says :—

"Those that are averse from assenting to any new discoveries but such as they can explain by an hypothesis, may for the present suppose, that as stones by falling upon water put the water into an undulatory motion, and all bodies by percussion excite vibrations in the Air; so the Rays of Light, excite vibrations in the refracting

Medium or Substance . . . that the vibrations thus excited are propagated in the refracting or reflecting Medium or Substance, much after the manner that vibrations are propagated in the Air for causing Sound, and move faster than the rays so as to overtake them . . . and that every Ray is successively disposed to be easily reflected or easily transmitted, by every vibration which overtakes it. But whether this Hypothesis be true or false I do not here consider."

In what follows he contents himself by describing the experimental facts. But his hypothesis, the guess of genius, after a century of discredit, is reasserting its superiority over the more logical, but less inspired, ideas of the early nineteenth century.

[G. P. T.]

GENERAL MONTHLY MEETING,

Monday, July 2, 1928.

Sir Arthur Keith, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Edwin Ernest Jelley,
Arthur Marshall, F.I.C.

were elected Members.

The Secretary reported the death of Professor Theodore William Richards on April 2, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss the Institution has sustained by the death of Theodore William Richards, M.A. D.Sc. LL.D., Foreign Member and Davy Medallist of the Royal Society; Honorary Fellow, Faraday Lecturer and Medallist, of the Chemical Society; Franklin Medal (1916), Lavoisier and Le Blanc Medals (Paris 1922), Officier de la Legion d'Honneur; Professor of Chemistry and Director of the Wolcott Gibbs Memorial Laboratory, Harvard University; Honorary Member of the Royal Institution.

Professor Richards' revision of the Atomic Weights of the Elements, for which he was awarded the Nobel Prize for Chemistry in 1914, has been of fundamental importance in the field of Physical Chemistry, and his discovery of two kinds of lead of different atomic weights laid the foundations of subsequent investigations of the bodies known as Isotopes.

The Managers desire to express, on behalf of the Members, their sincere sympathy with the family in their bereavement.

The following Lecture Arrangements for November and December, 1928, were announced:—

The Tyndall Lectures will be delivered on *Tuesdays*, Oct. 30, Nov, 6, 13.

Sir William Bragg, K.B.E. LL.D. F.R.S. M.R.I., Fullerian Professor of Chemistry. Four Lectures on *Tuesdays*, Nov. 20, 27, Dec. 4, 11.

E. D. Adrian, Esq., M.D. F.R.C.P. F.R.S., Lecturer in Physiology, University of Cambridge. Two Lectures on The Mechanism of the Nerves. On *Thursdays*, Nov. 22, 29.

Sir Richard Paget, Bart., F.Inst.P. M.R.I. Two Lectures on Human Speech. On *Thursdays*, Dec. 6, 13.

Rev. Theodore E. R. Phillips, M.A. F.R.Met.S., President of the Royal Astronomical Society. Two Lectures on Recent Discoveries respecting the Planets. On *Saturdays*, Nov. 3, 10.

Sir Hugh Allen, M.A. Mus.Doc., Director of the Royal College of Music. Three Lectures on a Musical Subject (with Musical Illustrations). On *Saturdays*, Nov. 17, 24, Dec. 1.

Walter Bayes, Esq., A.R.W.S., Head of the Westminster School of Art. Two Lectures on an Art Subject. On *Saturdays*, Dec. 8, 15.

The following Presents received since the last Meeting were laid on the table, and the thanks of the Members were returned for the same :—

FROM

International Standard Electrical Corporation—Comité International des Communications Telephoniques—Plenary Session, 1927. 4to. 1927.

Lick Observatory—Publications, Vol. XVI.—Stellar Radial Velocities. 4to. 1928.

Parker, W. Rushton, Esq., M.D. M.R.I.—Gerar. By Sir Flinders Petrie. 4to. 1928.

Qau and Badari I. By G. Brunton. 4to. 1927.

(British School of Archæology in Egypt Publications, Nos. 43–44.)

Rockefeller Institute for Medical Research—Studies, Vol. LXIII. 8vo. 1928.

The following communication relating

An Incident in Connection with Faraday's Life

was received by the Managers from Sir Alfred Yarrow, Bart., F.R.S. M.R.I., on the 2nd of July, 1928, and ordered to be inserted in the Proceedings of the Royal Institution.

I am one of the survivors, possibly the only survivor, who as a boy attended Faraday's Juvenile Lectures. At the end of the lectures at the Royal Institution, Faraday used to stop sometimes for quite half an hour talking to a lot of us boys, and sometimes making us go through some of his own experiments with our own hands.

A friend of mine of the name of Walter Rutt and I were so much impressed with Faraday's personality, that it was our practice on Sundays to meet Faraday and his wife at the corner where he used to go from the Caledonian Road up the hill to the road leading to Barnsbury, where at the corner we would stop and wait to raise our hats to him, which salute he always graciously returned. We then ran down a side street and then one parallel to the one taken by Faraday and his wife and up another street which intersected the road which Faraday passed over, and we saluted him a second time. We then frequently followed him to his chapel. He was a follower of John Glas and Sandeman, and attended a service which seemed to us very similar to a Quakers' Meeting. This was our regular job on Sunday afternoons. Perhaps I might take the liberty of saying that Faraday seemed much more at home at the Royal Institution than he did addressing the congregation, which devolved upon anyone who thought they should give an address.

(Signed) **A. F. Yarrow.**

WEEKLY EVENING MEETING,

Friday, February 24, 1928.

SIR ROBERT ROBERTSON, K.B.E. M.A. F.R.S., Honorary Secretary and Vice-President, in the Chair.

GEORGE GORDON,
Merton Professor of English Literature, University of Oxford.

The Lives of Authors.

[ABSTRACT DEFERRED.]

WEEKLY EVENING MEETING,

Friday, March 30, 1928.

SIR ALMROTH WRIGHT, K.B.E. C.B. M.D. F.R.S.,
Vice-President, in the Chair.

SIR ERNEST RUTHERFORD, O.M. LL.D. Pres.R.S. M.R.I.,
Professor of Natural Philosophy, R.I.,
and Cavendish Professor of Experimental Physics,
University of Cambridge.

Radioactive Atoms and their Structure.

[ABSTRACT DEFERRED.]

WEEKLY EVENING MEETING,

Friday, June 1, 1928.

SIR ARTHUR KEITH, M.D. LL.D. F.R.S., Treasurer
and Vice-President, in the Chair.

C. LEONARD WOOLLEY.

The Results of the Further Excavations at Ur.

WHEN the joint expedition of the British Museum and the Museum of the University of Pennsylvania resumed the excavations at Ur, digging was commenced on virgin soil. Near the surface were found graves of a late period, about 2700 or 2600 B.C.; slightly lower were graves of the first dynasty of Ur, about 3100 or 3000, and below these others, the contents, relative depth, and other conditions of which placed them about 400 years behind the first dynasty. The upper graves showed a custom prevalent both in the Sargonic period (2700 or 2600 B.C.) and in the earlier first dynasty. There is usually found in them, lying in the soil near the body, a boat made of a mixture of bitumen and earth, roughly modelled by the fingers, and round the boat clay vases which originally contained food for the dead. This custom, which does not appear at any later date, but which has its origin in very early antiquity, is of interest as implying that the passage to the next world was, according to early Sumerian belief, by water, bringing us at once into touch with early Egyptian beliefs and forming one more link between Egypt and Mesopotamia in the beginnings of their history.

The normal graves discovered at Ur consist of rectangular shafts of various depths; at the bottom of the shaft is placed the body, either wrapped in a piece of matting or enclosed in a coffin of basket work or wood, the earth shaft being only slightly bigger than the coffin itself. One rather larger than the rest was that of the Prince Mes-Kalam-Dug, which had been dug down at a later period into the original shaft of a great royal chamber. In the case of the royal graves the shaft is much larger, with one or more chambers solidly built in stone with vaulted roofs, entered by an arched doorway, at the bottom. They also revealed an entirely different custom, the main interment in each case having been accompanied by human sacrifice on a very large scale, clearly showing them to be royal graves as distinct from the commoners' graves elsewhere in the cemetery. The grave of one king contained the bodies of sixty men victims. The tomb immediately above it was probably that of his queen. As a whole series of graves of the first dynasty was found to have been dug down into the shaft of the latter, it is certain that this royal grave is earlier than the first of the intrusive burials.

In the small shaft burial grave of Mes-Kalam-Dug many objects of note in stone, metal and terracotta were found in the narrow space between the coffin and the wall of the shaft; vases and bowls in copper and silver, one in gold, daggers, many spears with copper blades and one with a gilt shaft; arrows mostly with heads of copper, but one quiverful with flint heads, and vases of alabaster and of clay. Inside the coffin itself was found the finest discovery yet made at Ur. The coffin had left only a black stain on the soil; the body was almost entirely decayed, but with it were a golden dagger and a golden lamp and bowls inscribed with the name of the prince, which might be translated as "the good lord of the land." Against the body was a tumbled mass of bracelets, earrings and beads, armlets in gold and lapis lazuli and carnelian, and resting on the arms and skull the golden wig-like helmet which the prince had presumably worn in war. The helmet was life-size, of beaten gold, the locks beaten up in repoussé, with the detail of the hair finely chased upon it.

The tomb of Queen Shubad was of an entirely different character. At the end of the shaft was a stone-built chamber vaulted with brick, in a recess was a paving of white stone slabs, possibly the slaughter place of the human victims whose bodies were spread over the whole of the remaining area. The bodies of five men, presumably the guard, were found at the door. There was the queen's chariot or sledge drawn by two asses whose bones were beside it, with their great collars of decorated copper, the bodies of the grooms lying at the animals' heads. Laid out in two rows were the bodies of women with elaborate headdresses of gold ribbon, their heads were bound with wreaths of carnelian and lapis lazuli beads, from which hung gold mulberry or lime leaves. The queen's wardrobe chest was found with its keeper beside it, and an enormous number of objects, vessels of silver and copper, stone and gold, including one perfect cup of lapis lazuli and an oval bowl ground out of obsidian, also the queen's gaming board, a heap of silver drinking pots, and a small group of vessels of gold. At the far end of the chamber was a harp inlaid with shell and lapis lazuli. In the queen's chamber was another mass of objects, vessels of copper, clay, silver and gold, many of which had been ranged on shelves against the walls. At one end on a wooden bier lay the body of the queen herself with two attendants crouched by the bier side, one at the head and one at the foot. The upper part of the body was entirely concealed by a mass of beads in carnelian, lapis lazuli, gold and agate; the beads had formed a short cloak which hung from the queen's shoulders to her waist, apparently open over the right arm and there held together by long gold pins with lapis lazuli heads; on the arm or on the joint of the cloak there were amulets of gold and lapis lazuli in the shape of fish and of gazelles. On her head she wore a very elaborate composite headdress of gold, broad gold ribbons wound round and round what was evidently a large and elaborate wig. Above that wreath after wreath of beads and golden pendants, the top wreath adorned with large gold flowers with petals inlaid with white shell and lapis lazuli, and on the top of the head a great golden comb decorated with rosettes.

The king's chamber, below the queen's grave, had been plundered, but contained further evidence of the primitive custom of human sacrifice at the death of a king. At the entrance were the bodies of six soldiers still wearing their copper helmets and carrying their spears. At the foot of the incline to the chamber were two carts or wagons of wood each drawn by three oxen, which had evidently been backed down the slope and the oxen killed there in position with the grooms at their heads. Against the wall of the chamber were nine ladies of the court wearing elaborate golden head-dresses. Other soldiers, with copper daggers at their belts, lay in a row, and more women were lying against the tomb side. The original doorway into the chamber was unbroken; the robbers having made their way down through the vaulted brick roof. The doorway was arched in brick, constructed upon the true voussoir principle. The actual chamber was rectangular, but by means of pendentives the square is brought to a curve and the end is roofed with a somewhat primitive form of half dome. The chamber produced a few objects of great value; the most important was a beautiful silver model of a boat, quite different in character from the bitumen boats in the graves of commoners; the type is similar to that found at the present time in the marshes of Mesopotamia—a long shallow craft with five seats, with leaf-bladed oars, and amidships an arch to support a mat awning. The six oxen harnessed to two carts at the bottom of the shaft are the earliest instances we have of domesticated cattle; of the carts themselves not much survived, just a black stain on the soil left by the decayed wheels and axles, but they were important documents for the antiquity of wheeled vehicles. Another remarkable object was the statue of a bull in wood with a head of gold and lapis lazuli and with shell plaques reaching from the animal's chin to its feet.

In the last grave of all, the deepest and earliest, the roof was not constructed on the true arch principle, but by corbelling out the stones. It had been hopelessly plundered and stray beads and bits of shell carving were scattered about promiscuously, only a few objects of real value being found, which included one simple little gold cup and two models of ostrich shells, one in silver and one in gold, decorated in somewhat barbaric fashion with incrustations in lapis lazuli and mother of pearl. In the very corner of the grave lay the flattened remains of a human skull, and by it a mass of mosaic which eventually proved to be a highly decorated box-like standard, composed of two pieces of wood fastened together back to back and inlaid with mosaic work in red stone, lapis lazuli and shell, set in bitumen. In various panels are seen the royal family sitting at a feast, servants bringing up the materials for the banquet, the Sumerian army with chariots, heavy troops and skirmisher troops, and prisoners being brought into the presence of the king.

[C. L. W.]

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